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OF THE

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FOR THE YEAR MDCCXCII.

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ERRATA.

PART I.

Page 52 line 9 - after water, *add* and ice.
—— 71 —— 12 - *for* bulb, *read* bottle.
—— 117 —— 6 - *dele* and.
—— 155 —— 21 and 27, *for* maggot bee, *read* bee maggot.
Meteorological journal, p. 26, l. 11, *for* 32,5, *read* 52.

PART II.

Page 314 line 27 - - *for* 0,36″ *read* 0″,36
—— 317 —— 27 - - 2,6″ — 2″,6
—— 326 —— 27 - - K — *k*
—— 347 —— 11 - - 105 — 10,5
—— 348 —— penult. - 105 — 10,5
—— 351 —— 15 - - $10\frac{1}{2}$ — $10\frac{1}{7}$
—— 355 —— 3 - - 0,94″ — 0″,94

IX. *On the Conversion of the Substance of a Bird into a hard fatty Matter. In a Letter from* Thomas Sneyd, *Esq. to Sir* Joseph Banks, *Bart. P. R. S.*

Read March 29, 1792.

SIR,

I TAKE the liberty of sending you two or three pieces of a bird whose substance has been converted into a hard fatty matter, which I found at the head of a fish pool, where a small brook runs into it, lying under water upon the mud. When first taken out, it was almost entire, and had several feathers sticking in different parts of the skin, which have since fallen out; a little down, however, still adheres to the smaller specimen. From the size, and general appearance of the bird, I conjectured it to be a duck, or young goose; but before I had time to give it a particular examination, it was unfortunately broken in pieces, and the greatest part destroyed. The skin in the piece which was saved is of different thicknesses, in some parts a full quarter of an inch; it has retained its original

structure exactly, but is in great part separated from the flesh, though both of them are now composed of the same fat matter. This substance resembles spermaceti in its consistence between the teeth, but has neither taste nor smell; it melts in a small heat, and when congealed again, becomes more solid, and looks like wax; in a greater heat it burns, and emits a strong animal smell. As I never heard or perceived that the water in which this bird lay has any particular property, I am inclined to think that it has undergone this singular change while buried in the mud, and that the brook had afterwards washed it up, and carried it into this pool.

I am sorry that the specimen and my account of this singular metamorphosis are so imperfect. The analogy which the case bears to the change of human bodies, observed by M. FOURCROY * in the Cemetery *des Innocents*, is my chief reason for offering them to you; and if they should be deemed worthy of the notice of the illustrious Society over which you preside, you are at liberty to present them.

I am very respectfully, &c.

THO. SNEYD.

Belmont, near Leek, Staffordshire,
March 16th, 1792.

* *Annales de Chimie*, Tom. V. p. 154.

X. *An Account of the remarkable Effects of a Shipwreck on the Mariners; with Experiments and Observations on the Influence of Immersion in fresh and salt Water, hot and cold, on the Powers of the living Body.* By James Currie, *of Liverpool, M. D. Fellow of the Royal College of Physicians at Edinburgh. Communicated by* Thomas Percival, *M. D. F. R. S.*

Read April 19, 1792.

THE following narrative is submitted to the Royal Society, as containing in itself some curious circumstances, and as having suggested the experiments afterwards to be recited.

On the 13th of December, 1790, an American ship was cast away on a sand-bank that lies in the opening of the river Mersey into the Irish Channel. The crew got on a part of the wreck, where they passed the night; and a signal which they made being discovered next day from Hillberry Island, a boat went off, though at a great risk, and took up the survivors. The unfortunate men had remained twenty-three hours on the wreck; and of fourteen, the original number, eleven were still alive, all of whom in the end recovered. Of the three that perished, one was the master of the vessel; another was a passenger who had been a master, but had lost or sold his ship in America; the third was the cook. The bodies of these unfortunate persons were also brought off by the men from Hillberry Island, and were afterwards interred in Saint

Nicholas church-yard, amidst a great crowd of spectators. The cook, who was a weakly man, died a few hours before the boat reached the wreck, but the two masters had been long dead, and this added to the sympathy for their loss, a curiosity to inquire into its circumstances and causes. When the following particulars came to be known, this curiosity was increased. Both the masters were strong and healthy men, and one of them a native of Scotland, in the flower of life, early inured to cold and hardships, and very vigorous both in body and mind. On the other hand, several of the survivors were by no means strong men, most of them were natives of the warm climate of Carolina, and what was singular enough, the person among the whole who seemed to have suffered least was a negro.

What is extraordinary is seldom long unaccounted for in one way or other, and the death of the two masters was said to have been owing to their having taken possession of a keg which had contained cherry-brandy, and which still contained the cherries;—these, it was reported, they had kept to themselves, and eaten in large quantities after the shipwreck; and this having produced intoxication, was supposed to have hastened their death. Some experienced seamen were satisfied with this account, which indeed seemed very rational; for though spirituous liquors may fortify the body against the effects of heat combined with moisture, and may perhaps support it for a short time under great fatigue, they are, I believe, uniformly hurtful when taken under severe and continued cold. Pleased to see a doctrine becoming popular which has been so ably supported by Dr. AIKEN,* and others, I be-

* See Transactions of the Philosophical and Literary Society of Manchester, V. I.

lieved it might receive a striking confirmation from this catastrophe, into the particulars of which I determined to examine accurately. I therefore obtained access to the survivors of the crew, and from them, but more especially from Mr. AMYAT, the mate, I received the information which I required.

In repeated conversations with this intelligent young man, I learnt that Capt. SCOTT, the master of the vessel, died in about four hours after the ship struck; and that Capt. DAVISON, the passenger, died in about seven: but that the incident of their having eaten cherries infused in brandy was entirely without foundation: of this he was certain, for he saw the keg, which contained the cherries, staved, while Capt. DAVISON was endeavouring to fill it with water to make grog for the crew; the cherries fell on the wreck, and were immediately washed into the sea. Mr. AMYAT expressed his surprise at the early death of the two masters, but could not assign any cause for it. He said there was no liquor of any kind saved, nor any sort of food; that the whole crew were on an equality in all points, except that some were deeper in the water than others, but that the two masters had the advantage in this respect, for they sat on the only part of the wreck that was out of the sea, whereas the poor negro, who escaped almost unhurt, was perhaps deepest in the sea of any. He explained this in the following manner. When the ship struck they cut away her masts to prevent her from oversetting, and after this she drifted over the sand bank, into what he called a "swash" on the other side. Here she floated, and they let go their best bower anchor, but it dragged, and the vessel struck again in a few minutes on another bank. In this situation she lay some time, beating against the sand, and the sea breaking over her. In

a little while Mr. AMYAT saw the tar barrels, which formed her cargo, floating towards the land, and soon after the bottom parted entirely, and was carried in the same direction. Happily for the men, the part of the wreck on which they were lashed was held by the anchor, and floated in the water, a small portion of the after part of the quarter-deck being above the surface. On this sat the two masters, generally out of the sea, but frequently overwhelmed by the surge, and at other times exposed to heavy showers of sleet and snow, and to a high and piercing wind. The temperature of the air, as nearly as can be guessed, was from 30° to 33° of FAH. and that of the sea, from trials in similar circumstances, from 38° to 40°. Immediately before the two masters was Mr. AMYAT himself. As he was sitting, and the deck sloped pretty rapidly, he was generally up to the middle in the water. The situation of the rest may be supposed; some of them were up to the shoulders. They were not at any time able to change their position, but kept their legs in pretty constant motion to counteract the cold, their arms being employed in holding by the wreck.

The master of the ship, Capt. SCOTT, a native of North-Carolina, and about forty years of age, died first. As they were in the dark, Mr. AMYAT could not see his countenance; but he was first alarmed by hearing him talk incoherently, like one in the delirium of fever. By degrees his voice dwindled into a mutter, and his hearing seemed to fail. At length he raised himself up in a sort of convulsive motion, in which he continued a few seconds, and then fell back dead on the deck. This happened about eight in the evening; four hours after the ship went aground. Soon after this, Capt. DAVISON, who

was about twenty-eight, began to talk incoherently, in the same manner as the other. He struggled longer, but died in the same way, at about eleven at night. The cook died in the forenoon of the succeeding day. He was a low-spirited man, and desponded from the beginning. All the rest held out, as has been already mentioned, though sorely pinched with cold and hunger, till they were taken up about three in the afternoon. Mr. AMYAT said that his hands and feet were swelled and numb, though not absolutely senseless; he felt a tightness at the pit of his stomach, and his mouth and lips were parched; but what distressed him most was cramps in the muscles of his sides and hips, which were drawn into knots. Though immersed in the sea, they were all of them very thirsty; and though exposed to such severe cold, Mr. AMYAT himself was not drowsy, nor were any of the men drowsy, nor did sleep precede death in those that perished. These facts are curious.

Reflecting on the particulars of this melancholy story, there seemed no doubt that the death of the two masters was to be imputed to their peculiar position on the wreck. Exposed to heavy showers of sleet and snow, they might suffer from being wet with fresh, rather than salt water: they might also suffer from being exposed to the cold of the atmosphere, probably seven or eight degrees greater than that of the sea. The chilling effects of evaporation might operate against them, promoted as these must have been by the high wind; or they might receive injury from their frequent immersions in the sea, producing an *alternation* in the media surrounding. This last supposition did not, indeed, strike me at this time; the others dwelt on my mind.

Of the powers attending animation, that which seems fundamental, is the capacity of the living body of preserving the same heat in various degrees of temperature of the same medium, and, indeed, in media of very different density and pressure. If a definition of life were required, it is on this faculty that it might best be founded. It is known that some fluids, applied to the skin, vary in their effects according to their impregnation. In the same degree of temperature, pure water on the surface of the body is much more hurtful than water in which salt is dissolved. Seafaring men are universally acquainted with this, and a striking proof of the truth, as well as of the importance of the observation, may be found in the Narrative of Lieut. BLIGH. Probably the saline impregnation may stimulate the vessels of the skin in some way that counteracts the sedative or debilitating action of the cold. At any rate, it seemed not unlikely that some light might be thrown on this curious subject, by observing the effects of immersion in fresh and salt water, of equal temperature, on the animal heat. And this might also assist in accounting for the death of the unfortunate men already mentioned.

EXPERIMENT I.

I placed a large vessel, containing one hundred and seventy gallons of salt water, in the open air. The atmosphere was damp, and what is called raw. The thermometer stood at 44° in the air, and this also was the temperature of the water. The subject of my experiment was RICHARD EDWARDS, a healthy man, twenty-eight years of age, with black hair, and a ruddy complexion. The hour chosen for his immersion was four in the afternoon, about two hours after his dinner; a time

appointed rather for my own convenience, than as being most proper for the purpose. His heat was 98° before undressing, his pulse 100 in the minute. He was undressed in a room where the mercury was at 56°; and afterwards stood naked before the fire till his heat and pulse were examined again, and found as before. He then walked pretty briskly through a flagged passage into an open court, where the north-east wind blew sharply upon him: he was exposed to it for a minute, and then plunged suddenly into the water up to the shoulders. The thermometer, which had been kept in a jug of warm water, at the heat of 100°, was introduced into his mouth, with the bulb under his tongue, as soon as the convulsive sobbings occasioned by the shock were over. The mercury fell rapidly, and a minute and a half after immersion it stood at 87°. He remained motionless in the water, and the mercury rose gradually; at the end of twelve minutes it stood at $93^{\circ}\frac{1}{2}$. While he sat in the water, it occurred to me to examine his heat when he rose out of it into the air: I had reflected on the power that must be employed to keep up his heat in a medium so dense as water, and where an inanimate body, of the same bulk, would have cooled so much more speedily than in air of the same temperature. Supposing that this heat-producing process, whatever it may be, might continue its operations some time after the extraordinary stimulus (the pressure of the water) was removed, I expected to see the mercury rise by the accumulation of his heat, on changing the medium of water for air, and therefore kept him exposed, naked, to the wind two minutes after taking him out of the bath. To my surprise, although the attendants were rubbing him dry with towels during this time, the mercury fell rapidly. He was put into a warm bed, and his heat, when examined under

the tongue, was 87°, at the axilla 89°. Frictions were used, and brandy mixed with water administered; but I found on this, as on all future occasions, that the best mode of counteracting the cold, was to apply a bladder, with hot water, to the pit of the stomach (the scrobiculus cordis), a fact which I think important: this being done, his shiverings, which before were severe, soon ceased, and he became more comfortable. Three hours afterwards, however, he had not entirely recovered his former heat; but by eight at night, he was in all respects as usual.

I have been very minute in detailing the circumstances under which this experiment was made; some of the particulars which, at the time, I thought of little consequence, I found afterwards of importance. The experiment itself I determined to repeat as exactly as possible.

EXPERIMENT II.

On the next day, at the same hour, the same person was again immersed, as before. His pulse previously was 85, his heat 100°. He had been put to bed an hour before, to save the time spent in undressing. The heat of the water and of the atmosphere 44°. The wind north-east, and strong. On this occasion, as before, there was a rapid fall of the mercury; the following table will save words:

	Ther.		Ther.
2 min. after immersion	89°½	9 min. after immersion	95°¾
3 ——	90½	10 ——	94½
4 ——	92½	11 ——	95
5 ——	94½	12 ——	95
6 ——	95	13 ——	95½
7 ——	95¾	14 and 15 ——	95
8 ——	95¾		

At the end of fifteen minutes he was taken out, and stood three minutes, naked, exposed to the north-east wind, at the end of which time the mercury had sunk to 88°. A draught of ale was given him, and he was put into a warm bed; in three minutes after the mercury rose to 93°. An hour after his heat was 95°.

The effects produced by this alternate exposure to water and air of the same temperature, gave a new direction to my thoughts, and determined me to inquire again into this singular phænomenon. The most obvious method would have been to have prolonged the process of alternation, and replunged the person cooled by the external air into the bath; but this was running too great a risk, unless some more sudden and certain method could be found of restoring the heat that might be lost. It was prudent, therefore, to proceed more cautiously. In the next experiment I resolved to try the methods of heating as well as cooling the body.

EXPERIMENT III.

On the following day, at the same hour, the same person was again immersed in the salt-water bath. His heat previously was 98°, his pulse 100. The temperature of the air and the atmosphere, as before, 44°. The mercury sunk rapidly to 90°.

2 minutes after	-	88°	10 minutes after -	94° ½
3 ———	-	88	11 ——— -	94 ¾
4 ———	-	88 ½	12 ——— -	95
5 ———	-	90 ½	13 ——— -	96
6 ———	-	92	14 ——— -	96
7 ———	-	92	15 ——— -	96
8 ———	-	94	16 ——— -	96
9 ———	-	94		

He was now taken out, and stood in the wind three minutes, shivering violently. This circumstance rendered it difficult to ascertain exactly the fall of the mercury, which was, however, considerable. When examined in the room in which he undressed, it stood at 90°. He was now plunged into a fresh-water warm bath, heated to 97°½. What is very surprising, the mercury fell two degrees. The following table will show the progress of the return of his heat.

1 min. after immersion in the warm bath, mercury - 88°	5 minutes after - 94°
2 minutes - 92	6 ——— - - 96
3 ——— - - 92	7 ——— - - 96
4 ——— - - 94	8 ——— - - 96
	9, 10, 11, 12, to 16, 96

If the rise of heat in the cold bath at 44°, and the warm bath at 97°½, be compared, the first will be found more slow; but that after being sixteen minutes in the one and in the other, the heat was the same in both cases, when taken at the mouth. It must, however, be acknowledged, that in the cold bath, the extremities were chilled and cold, while in the hot bath, the heat was equally diffused. When EDWARDS got out of the hot bath, he put on his clothes, and was remarkably alert and cheerful the whole evening. Encouraged by the safety of these experiments, I resolved to increase the time of immersion in the cold bath, and to inquire more generally into its effects on the sensations, as well as heat.

EXPERIMENT IV.

At the same hour of another day, the same person was again immersed as before, his heat previously being 97°½, and that of the water 42°. Wind north-east, and brisk.

1 minute after,	heat 90°	12 minutes -	
2 minutes -	92	13 ——— - -	
3 ——— - -	92	14 ——— - -	94°½
4 ——— - -	92 ½	15 to 24, - -	94 ½
5 ——— - -	92	25 ——— -	94
6 ——— - -	92 ½	26, 27 - -	
7 ——— - -	94	28 ——— -	94 ½
8, 9, 10, 11 -	94	29, 30 -	94

It will be observed, that in the above table there are blanks left in the report. At such times the thermometer was taken out of EDWARDS's mouth, to admit of his answering the questions put to him. He said, that on plunging into the water he felt an extreme cold, which he could not but think was partly owing to his being exposed, naked, to the wind before; that this cold diminished, and in a little while he felt comfortable, but that after a while the sense of coldness returned, though less than at first; diminishing again, but in a less degree. At length his sensations became pretty fixed. In this state, when the water was at rest, he should not even have known, by his feelings from the upper part of his chest to the pubes, that he was in water at all. His feet and legs were very cold; so were his hands and arms; and so also the penis and scrotum. He mentioned, likewise, that he felt a cold circle round the upper part of his body, though not constantly. On examining into this, I found it was greatest at first, and that it extended over the space which, from the undulations left in the bath by the plunge of immersion, was alternately above and under the surface of the water: when the bath settled, it was little felt; but by agitating the fluid, I could reproduce it, at any time when the cold in the extremities was not so great as to prevent its being felt. This curious parti-

cular serves to explain a circumstance much dwelt on by Mr. AMYAT, in giving an account of his sufferings on the wreck; that what he felt most severely was the cramps in the muscles of his hips and sides, parts which, from his situation on the wreck, already described, must have been alternately under and above the surge. Here I must observe, that the sea did not break over the sufferers all the time they were on the wreck. The wind moderated, as well as the waves, and for the last fifteen hours, they were not at any time overwhelmed, or at least Mr. AMYAT himself was not. The cold never abated. Being all lashed to the wreck, they never changed their positions; the bodies of those who died occupied the space where they were originally placed. Mr. AMYAT, therefore, during the whole time sat nearly up to the middle in water, but subject to the variations occasioned by the motion of the sea.

To return.—When exposed naked to the wind, the mercury, in this case, sunk as usual five or six degrees, and his shiverings were great. Desirous of restoring his heat as speedily as possible, we incautiously heated the hot bath to 104°: but after being half a minute in it, he screamed out with pain, especially in his extremities, and about his scrotum. When taken out, his shiverings almost amounted to convulsion. The bath was lowered to 88°, and he was replaced in it, and its temperature progressively, but pretty rapidly, increased to 100°. He continued, however, to shiver much, his heat remaining about 90°; but a bladder, with very hot water, being introduced under the surface of the bath, and applied close to his stomach, the good effects were instantaneous, his shiverings ceased, and his heat mounted rapidly to 98°.

All these experiments having been made on one person, I determined to repeat this last on another.

EXPERIMENT V.

R. SUTTON, æt. 19, of a pale complexion, and a feebler frame, was immersed in the bath, under the circumstances of the preceding experiment. His heat was previously 96°½.

½ a minute after,	heat 92°	18 minutes	93°¼
1 minute	90	19 ———	93½
2 minutes	88½	20, 21	94
3 ———	89	22 ———	92½
4 ———	90	23 ———	92¼
5 ———	92	24 ———	92¼
6 ———	92¼	25 ———	94
7 to 10	92	26 ———	94
11 ———		27 ———	92½
12 to 15	92	28 ———	92¼
16 ———	92¼	29 ———	94
17 ———	93	30 ———	94

Though this person seemed to bear the cold bath well, having lost in 30 minutes only 2½ degrees of heat, yet when exposed afterwards to the wind, he shivered violently, and lost his heat very fast. He was put into a warm bath, heated to 96°, but recovered his heat very slowly, as the following table will show.

1 minute after,	heat 88°	
2 minutes	90	
3 ———	90½	
4 ———	90	great shivering.
5 ———	90	here the bath was heated to 100°.

6 ——	90°	shiverings still.
7 ——	90	ditto.
8, 9	90½	ditto.
10 ——	92	ditto.
11 ——	92	bath heated to 104°.
12 ——	94	
13 ——	93	—— heated to 108°. Shiverings.
14 ——	93	a bladder with very hot water applied to the stomach.
15 ——	94	
16 ——	96	very comfortable.

EXPERIMENT VI.

RICHARD EDWARDS, the original subject of experiment, was again immersed in the cold bath, of the temperature of 40°, and remained in it three quarters of an hour. His heat previously was 97°; his pulse 90 in the minute. The mercury fell to 92°, was stationary for a few minutes, and then mounted, though, as usual, with no regularity. In twenty-two minutes it stood at 96°; it then began to decline, and in twenty-three minutes more had sunk to 94°. Being exposed as usual to the wind, the mercury sunk as usual, and he shivered violently. In the warm bath at 96° his shiverings continued several minutes, his heat remaining at 90 and 91°. In seven minutes the mercury began to rise fast, and five minutes after was at 96°.

EXPERIMENT VII.

The effects of forty-five minutes immersion in the cold salt-water bath, at 40°, were proposed to be tried on RICHARD SUTTON. He was much under the impressions of fear, and his heat previously raised the mercury only to 94°. The mercury sunk, as usual, on his immersion, but to an unusual degree. It did not stop in its fall till it got to 89°, which perhaps might be in part accounted for by the extraordinary chattering of his teeth, admitting some contact of the air. It then mounted in the usual irregular way, and at the end of thirteen minutes had got to 92°. Here it stood for nineteen minutes longer with little variation; at the end of this time it began to fall rapidly, though irregularly, and in three minutes was down at 85°. He had now been thirty-five minutes in the water, and I did not think it safe to detain him longer; we therefore hurried him into a warm bath, heated to 96°, where he shivered much. The bath was heated gradually to 109°, and in this heat he recovered his proper temperature in about twenty-eight minutes. Being then put into a warm bed, he fell into a profuse perspiration, which left him in his usual health.

One general remark will serve for the pulse in all these experiments. It was not possible to keep the subjects of them from some degree of previous agitation, and this always quickened the pulse. The natural pulse of EDWARDS was about 70 in the minute; but it may be observed, that it was never slower than 85 before immersion, and generally more. However this might be, it invariably sunk to 65, or from that to 68, in the water, became firm, regular, and small. After being long in the bath, it could hardly be felt at the wrist, but

the heart pulsated with great steadiness and due force. In the last experiment, when the heat sunk rapidly, SUTTON said that he felt a coldness and faintness at his stomach, which he had not perceived before, and when I felt the motion of his heart, it was feeble and languid. In some future trials of the effects of immersion in fresh water (one of which I shall detail), the same coldness at the stomach preceded a rapid fall of the mercury; and these facts, together with the effects I found from applying a considerable heat to this part when the body was chilled with cold, convince me that there is some peculiar connection of the stomach, or of the diaphragm, or both, with the process of animal heat. Whoever will consider the rapidity with which a dead body would have cooled immersed in water of the temperature of 40°, may form some estimate of the force with which the process of animal heat must have acted in the experiments already recited. These experiments, however, furnish irrefragable proofs of the futility of some of the theories of animal heat. The increase of heat, in fever, has led some persons to believe that animal heat is produced by, or immediately connected with, the action of the heart and arteries; here, however, it may be observed, that while heat must have been generated in the bath with more than fourfold its usual rapidity, the vibrations of the arterial system were unusually slow. Another, and a very beautiful theory of animal heat, supposes it immediately to depend on respiration; but in the bath, after the first irregular action of the diaphragm from the shock of immersion was over, the breathing became regular, and unusually slow. Lastly, the curious phænomenon of the heat rising, and falling, and rising again, in the bath, with the body at rest, and the temperature

of the surrounding medium unchanged, is, I think, fatal to those theories of animation which consider the living body as a mere machine, acted on by external powers, but not itself originating action, and differing from other machines only in the peculiarity of the powers which are fitted to set it in motion. I have said that the temperature of the medium continued unchanged, but it may be supposed that the bath was heated a little during the experiments; it was so; but being exposed, with a large surface, to the open air, the wind blowing briskly over it, its heat was little altered; in twelve minutes immersion it had gained nearly one degree, and in forty-five minutes, the longest duration of any of the experiments, it had gained three degrees. As this accession was regular, if it had been greater it would not have invalidated the foregoing observations.

Many other trials were made on the effects of immersion in water on the human heat, which I shall speak of generally, under the general conclusions which they suggested.

The experiments already recited, suggested to me the notion that in all changes from one medium to another of different density, though of the same temperature, there is a loss of animal heat. I found, however, that this conclusion requires many restrictions.

1. My experiments being made on bodies of such very different density as air and water, do not admit an universal inference of this sort.

2. Being all made in a temperature fifty degrees under the human heat, no certain conclusion can be drawn as to what might happen in degrees of heat much higher, where it is probable, the effects of the change, if it appeared at all, might be

less striking. It would seem, however, that after a person is long chilled in cold water, the first effect of passing through the external air into the warm bath, is first a fall of heat in the air, and after this a still greater fall in the warm bath, followed, however, by a speedy rise.

The air and the water being equally cold, and both 45° or under, I found the loss of heat in passing from the one to the other to be regulated in the following way.

1. If, instead of being exposed naked to the wind previous to immersion in the water, the body was kept warm by a flannel covering, the mercury fell much less on the first plunge.

2. If, after plunging into the water, the person continued in it only a minute or two, a subsequent fall of the mercury did not always take place, on his emerging into the air. On the contrary there was sometimes a rise on such occasions in the mercury, especially if the atmosphere was at rest.

3. In one instance, after continuing in the water fifteen minutes, on rising into the air in a perfect calm, though during a frost, there was little or no seeming diminution of the heat; while exposure under similar circumstances, with a north-east wind blowing sharply, though the air was many degrees warmer, produced a rapid diminution. The effects of the wind in diminishing the human heat are indeed striking, and are not in my opinion explained by the common suppositions.

4. The loss of heat by a change of media, depends much on the rapidity of the change, for the plastic power of *life* in varying the process of animal heat, so as to accommodate it to the external changes, acts for a time with great celerity, though this celerity seems to diminish with the strength.

EXPERIMENT VIII.

I placed in a large room, where the mercury stood at 36° two slipper baths at the distance of six yards from each other. One was filled with cold salt-water of the temperature of 36°, the other with water heated to 96°, which was my own heat. Undressing myself in an adjoining room by a fire, I afterwards slipped on a loose flannel dress, and descended *slowly* into the cold bath, where I remained two minutes; I ascended *slowly* into the air, and then sunk myself in the warm bath, where I remained two minutes also: I returned to the cold bath, where I staid two minutes as before, and removed from it again to the warm bath. But during all these changes of media and temperature, the thermometer with its bulb under my tongue never varied from 96°. I attribute this partly to the heat of my body being in some degree defended by the flannel dress, partly to the calm of the air, but chiefly to the slowness of motion in these changes. It may be said that the time of staying in the different baths was not long enough to produce any sensible change in the heat of circulating fluids of such a mass, but this is not consistent with many of the other facts.

5. The influence of the application of cold water to the surface of the body on the heat, is in some respects regulated by the animal vigour, as the following experiment will show.

EXPERIMENT IX.

In the same room I placed a large empty vessel: in this two young men sat down in succession, each with the bulb of a thermometer under his tongue. A man standing on a bench

with a bucket of cold salt-water containing four gallons, poured the whole on the head and shoulders, suffering it to run down on the rest of the body. This process took up nearly a minute, during which I examined the mercury, and found it unchanged. They were both directed to continue sitting without motion for a minute after, during which, in both instances, the mercury rose two degrees. A third, much inferior in vigour, submitted to the same experiment, and the mercury continued during the affusion of the water unchanged, but in a minute after sunk half a degree. In fevers, where the heat is generally increased from two to six degrees above the standard of health, pouring a bucket of cold water on the head always reduces the pulse in frequency, and commonly lowers the heat from two to four or five degrees. Of this salutary practice I hope soon to speak at large to the public.

6. The power of the body in preserving its heat under the impressions of cold, and the changes of temperature, and of media, seems in some measure regulated by the condition of the mind. That fear increases the influence of cold, and of many other noxious powers, will not be doubted; but the state of the mind to which I allude, is that of *vigorous attention* to other objects. This, it is well known, will to a certain degree deaden, or, indeed, prevent the sensation of cold; and what does this, I apprehend, prevents, or at least weakens, its physical action. The astronomer, intent on the objects of his sublime science, it is said, neither feels, nor is injured by, the damps nor the chillness of the night; and in some species of madness, where the ideas of imagination are too vivid to admit the impressions of sense, cold is resisted to an extraordinary degree. I have seen a young woman, once of the greatest

delicacy of frame, struck with madness, lie all night on a cold floor, with hardly the covering that decency requires, when the water was frozen on the table by her, and the milk that she was to feed on was a mass of ice.

7th. There are particular conditions of the atmosphere, not perfectly understood, that seem to have an influence in depriving us more speedily of our animal heat, than others where the cold is greater.

It may seem that by this time I had renounced my intention of trying the effects of immersion in fresh water on the animal powers, and particularly on the heat. Some trials I have, however, made, of which I shall only relate the following.

EXPERIMENT X.

In the same vessel, containing an equal bulk of fresh water, RICHARD EDWARDS, the subject of my first experiments, was immersed, at the same hour of the day. His heat previously was 98°, his pulse beat 92 in the minute: the heat of the air was 41°½, that of the water 40°. The wind was now in the west, so that in the court where the bath stood there was a perfect calm. As I had some fears of the issue of this experiment, instead of exposing him for a minute naked to the wind before immersion, he was covered with a flannel dress from the air till the instant he descended into the water, into which he was suffered to sink himself slowly, with the bulb of the thermometer under his tongue. These are important circumstances. The following table exhibits the result.

Immediately on immersion, heat	98°	14 min. after, heat	96°½
1 minute after	97½	15	96
2 minutes	97	16, 17, 18, 19, 20	96
3 ———	98	21, 22, 23, 24	
4 ———	97½	25 ———	95
5 ———	96	26 ———	94
6 ———	96	27 ———	93¼
7, 8	96	28, 29	94
9 ———	97	30	93
10 ———	97	31, 32	94
11, 12, 13		33, 34	92½

He now got out into the air very slowly, and stood in it three minutes, the wind *not* blowing on him. He lost one degree of heat at first, which he recovered. He was then put into a warm bath at 90°, which at first *he felt* warm, and his feet and hands were pained: but in two minutes he fell into a very violent shiver, and his heat fell two degrees. The bath was then heated to 95 and 96°, but still he felt cold. It was heated to 99°: he continued in it five minutes, and his heat was 91°. The heat was gradually raised to 106°, when the sense of coldness of which he had complained at the pit of the stomach gradually went off. Before this I had usually kept him in the warm bath till his natural heat was nearly recovered, but after being half an hour in the heat of 106°, his own heat was still 93°. He now became sick and very languid, a cold sweat covering his face, his pulse very quick and feeble. He was removed into bed, but passed a feverish night, and next day had wandering pains over his body, with great debility, resembling the beginning stage of a fever. By cordials and rest this went off.

This experiment clearly enough confirms the greater danger of being wet with fresh than salt water; but in itself points out nothing certain besides, except that it is not to be rashly repeated. I mean to try some of these experiments to a greater extent on the brute creation, when I have procured thermometers better suited to my views. The thermometers I employed had not a sufficient mobility for very nice experiments, and I am well aware that in particular instances this may have misled me, though the general results, which is all that is of importance in such experiments as these, will, I hope, be found just and true.

Before I conclude, I must offer a few observations on the subject that led to these experiments.

1. It is, I think, already well known among seamen, that where there is only the choice of being wet with salt or fresh water, it is always safest to prefer the first. In the heavy showers of rain, hail, or snow, by which gales of wind are generally accompanied, the men that must be exposed to them, ought, like Lieutenant BLIGH and his crew, to wring their clothes out of salt-water.

2. In all cases where men are reduced to such distress by shipwreck or otherwise, that they can only chuse between the alternative of keeping the limbs constantly immerged in the sea, or of exposing them to the air while it rains or snows, or the sea is at times washing over them, it is safest to prefer a constant immersion; because, in the northern regions where the cold becomes dangerous to life, the sea is almost always warmer than the air, as the experiments of Sir CHARLES DOUGLAS shew; and because there is not only a danger from the increased cold produced by evaporation, but also from the loss

of heat by the rapid changes of the surrounding medium, as the foregoing experiments point out.

3. Whether, in high and cold winds without rain or snow, and where a situation may be chosen beyond the reach of the waves, it is safer to continue in the air, or to seek refuge in the sea, must depend upon several circumstances, and cannot perhaps be certainly determined. The motives for choosing the sea will be stronger in proportion as the wind is high and cold, and in proportion as the shore is bold.

The foregoing narrative shows that men may survive twenty-three hours immersion in the sea, of the temperature of 38° or 40° (as great a cold as it almost ever possesses) without food or water, and almost without hope of relief; but that any man ever survived an equally long exposure to the higher degrees of cold of the atmosphere, in the same circumstances, does not appear. Though in the case related, immersion in water did not prevent thirst, yet there is no doubt that it alleviated it, a circumstance of high importance towards the preservation of life.

Liverpool,
December 25, 1791.

P. S. I have purposely avoided any reasoning on the causes of the loss of vital heat on the change of media in the experiments recited. It may be supposed that during immersion, the water immediately in contact with the skin having become heated to a certain degree, the naked body, on rising from it into the air, was in fact exposed to a colder medium, and thus the loss of heat, in this instance, produced. My examination of the heat of the water during immersion not

having been made in contact with the body, I will not deny that there is some foundation for the remark; and the cases, it must be allowed, are by no means exactly parallel between immersion in an open vessel, however large, and immersion in the sea, where the constant undulation may be presumed to occasion a continual change in the surrounding fluid. But whatever allowance may be made for the circumstance mentioned, I am persuaded that the difference between the density of air and water being considered, it is not sufficient to explain the loss of heat in the instance alluded to. The changes of temperature in the living body are governed by laws peculiar to itself. I have found, in certain diseases, greater and suddener variations than any mentioned, from applications of cold very gentle in degree, and momentary in duration.

In his masterly "Experiments and Observations on Animals producing Heat," Mr. HUNTER has objected to taking the heat of the human body by introducing the bulb of the thermometer into the mouth, because it may be affected by the cold air in breathing. The objection is well founded if the bulb be placed on the upper surface of the tongue, but if it be under it and the lips shut, the effects of respiration may be disregarded, as I have found from many hundred experiments. The heat may be observed in this way with ease and certainty, by employing thermometers curved at that end to which the bulb is affixed (the bulb being introduced at the corner of the mouth), some of which have been made for me by Mr. RAMSDEN according to a form given, as well as others on Mr. HUNTER's plan. From repeated trials it appears to me, that when the usual clothing is on, the heat of the living body may be taken, with nearly the same result and equal

certainty, under the tongue with the lips shut, at the axilla with the arm close to the side, and in the hollow between the scrotum and the thigh; every other part of the surface is liable to variation and uncertainty. It is evident that of these three methods, the first only can be employed (as far as I can discover) when the trunk of the body is immersed in water; and even when the naked body is exposed to the cold air, the first method seems the best, the heat remaining most steady under the tongue: the axilla is the next best in order, and the worst, the lower part of the groin; for the scrotum and the parts of generation lose their heat on the application of cold more speedily perhaps than any other part of the body, the extremities not excepted.

N. B. The water employed in the experiments related, contained salt in the proportion of one to twenty-four.

Instead of saying that the men saved were most of them natives of Carolina, I find I ought to have said, men long accustomed to that country and other warm climates, but not most of them natives.

XI. *A Meteorological Journal, principally relating to Atmospherical Electricity; kept at* Knightsbridge, *from the 9th of* May, 1790, *to the 8th of* May, 1791. *By Mr.* John Read; *communicated by* Richard Henry Alex. Bennet, *Esq. F. R. S.*

Read April 26, 1792.

A DESCRIPTION of the instruments for collecting atmospherical electricity, used in the following journal.

Fig. 1 and 2 (Tab. IV.) represent the apparatus. AA fig. 1, is a round deal rod, 20 feet long, two inches diameter at the lower, and one inch at the upper end: into the lower end of it is cemented a solid glass pillar B, 22 inches long; the lower end of this glass stands in a socket of wood C, which is screwed on the garret floor D, and supports the whole. About seven feet above the floor, is firmly fixed to the wall a strong arm of wood E, which holds perpendicularly a strong glass tube F, through which the rod is slided gently upwards, till the glass pillar B may be lowered into the socket C. It is thus fixed, and stands 12 inches from the wall. The tube F is of sufficient width to admit a case of cork, which is fastened in the inside of it, at the part where the tube is sustained by the arm of wood E, so that the rod, when bent by the wind, cannot touch the tube or break it. The upper extremity of the rod is terminated by several sharp-pointed wires G; two of them are of copper, each one-eighth of an inch thick; and, in order to stiffen the rod, as well as conduct more readily the

electric fluid, one of them is twisted round the rod to the right hand, and the other to the left, as low down as the brass collar H, to which they are soldered, in order to render their contact perfect. A little above the upper end of the glass pillar B, is placed a brace II of solid glass sticks, to keep steady that part of the rod. K is a hollow cylinder of wood, 12 inches within, proceeding from the ceiling through the roof LL, above which there is a hollow tin cover M, 24 inches within, fixed to the rod; which serves to defend the open cylinder K from the weather. At a convenient distance from the foot of the rod is a hole bored through the floor and ceiling. This hole receives a glass tube covered with sealing-wax (as are all the other glasses in this apparatus), through which a strong brass wire proceeding from the rod is conveyed into the room below, wherein, at a convenient height from the floor, it terminates with a two-inch brass ball N. A strong ring of brass is made to move easy on the wire, and rests on the brass ball; into this ring is screwed a brass wire seven inches long, which keeps suspended at its extremity a pith-ball electrometer O.

At two inches distance of the above-mentioned brass ball N, a bell P is supported by a strong wire, which passing through a hole made in the wall, is made to communicate, by means of a good metallic continuation Q, with the moist ground adjoining to the house. A brass ball, three-tenths of an inch in diameter, is suspended between the bell P and ball N, by a silk thread fastened to a nail R. This ball serves for a clapper, by striking between the ball and bell, when the electrical charge of the rod is sufficiently strong. V gives nearly a front view of this part of the apparatus.

S is a small table fixed to the wall under the bell and ball, at a convenient height above the floor, upon which Leyden bottles and other apparatus are occasionally placed. Any person versed in the science of electricity, will easily understand that this apparatus is calculated to show the various degrees of intensity of atmospherical electricity; and at the same time to avoid the pernicious effects which may be occasioned by thunder-storms, or in short by any great quantity of electricity in the atmosphere.

The whole perpendicular height of both parts of this apparatus taken together, from the moist earth to the point at the top of the rod, is 61 feet. If the insulation could be constantly kept in due temperature, with respect to heat and cold, I imagine it would always be electrified. But I fear that cannot be done, without the aid of common fire; which in so large an apparatus would be very difficult, so that there might not be too much nor too little of it. I am inclined to this opinion, from the success I have had in a number of experiments, in which the aid of common fire was applied to improve the insulation, as well as to collect the electric fluid.

When I find that the moisture in the air has so much injured the insulation of my high pointed rod, that it will not retain a weak electricity, in that case I make use of my hand exploring rod, which is about the length and thickness of a common fishing-rod, with plenty of small wire twined round it from end to end. The method of using it is simple and easy. A representation of it may be distinctly seen in fig. 2. Having first *warmed* the glass legs of the stool, I place myself upon it, and raise the rod into a vertical position, keeping it so for a minute or two; I then with a finger

of the other hand touch a sensible electrometer, and if the threads open, it is sufficient. But should the electrical state of the atmosphere be too weak to produce that effect, which seldom happens, then in that case I add to the rod a *lighted torch* T, and place it as remote from my hand as the strength of the rod will bear, and repeat the experiment; thus circumstanced, it has never yet failed me.

It will be necessary just to mention the method I have pursued in forming the journal of atmospherical electricity. This has been principally by means of the signs exhibited by the pith balls O, connected with the rod. When I find these closed, and attracted on the approach of my finger, yet not sufficiently charged to repel each other, I write weak signs of electricity. When I find the balls open, and, on the approach of excited glass, the balls close, I write they are electrified positively; but, if the balls open wider, I write they are electrified negatively; and the reverse when I use sealing-wax. When the balls diverge one inch and upwards, visible sparks may be drawn at the brass ball N. When sparks are said to have been perceived in any observation, I have generally on that account omitted to note the variable quantities of divergency in the pith balls. Their utmost limit of regular divergency seems to be about five or near six inches; above that they are unsteady and disorderly. The pith balls are near two-tenths of an inch in diameter, suspended by very fine flaxen threads (in the state it is in from the heckle) five inches long. When I mention the distance of the balls in tenths of an inch, it is to be understood as nearly so as my eye can determine.

This apparatus requires a constant attention, especially

during a disturbed state of the atmosphere. From the room in which the apparatus is placed I am seldom absent one hour, excepting the time of sleep; but, when I leave it, the last thing I do at night is to examine the state of the electricity, and, if I find the rod unelectrified, I then place the Leyden bottle on the table S, with its knob nearly in contact with the ball N. The next morning, if I find this bottle charged, I write the kind of electricity it is charged with against the day in the journal, and add, *by the night bottle*.

It is presumed, that the table is sufficiently obvious. The two columns for positive and negative electricity are used only for the first observation of each day. I use FAHRENHEIT's thermometer, suspended on the north outside of a bow-window. The time of making the observation with it, and the barometer, and also of the direction of the wind, has usually been nine o'clock in the morning.

Lastly, it may be useful to observe, that I have always found, when the rod is highly electrified, the lower though *uninsulated* part of the apparatus (*viz.* the metallic connection of the bell P with the moist earth) to be in a contrary state of electricity to the upper and insulated part. See the 20th of May.

Having made a memorandum of the several thunder-storms which have happened in divers parts of this island, according to information by letters, and from newspapers, I thought it useful to insert them in this journal, in order to show whether some contemporaneous appearances in my apparatus might not be attributed to them.

Days.	Wind.	Barom.	Ther.	Sparks.	Pos.	Neg.	May 9, 1790.
		Inches.	°				
May 9	N	29.14	67	strong	pos.	—	A clear fine morning. One o'clock P.M. a sudden shower of rain fell; the pith balls O, connected with the lower end of the rod, were now charged negatively, and increased in strength as the rain came on; in a short time the electricity changed to positive. Five o'clock, the electricity changed again to negative, I then heard some distant rumbling of thunder. This day there was some lightning at Hertford.
10	NE	29.18	66	o	—	neg.	A.M. sky very cloudy, and some small rain falls.
11	E	30. 5	64	o	pos.	—	Strong gales of wind; and a weak electricity.
12	E	29.14	70	o	pos.	—	
13	E	29. 6	72	o	pos.	—	
14	NE	30. 9	72	o	pos.	—	
15	N	29. 5	65	o	pos.	—	A.M. weather serene.
16	NE	29. 2	57	o	pos.	—	A.M. a clear sky. Some lightning at Bishop-Stortford.
17	S	29. 8	58	strong	pos.	—	Half after nine o'clock A.M. fell a little rain; as it increased so did the electric charge in the rod, insomuch that it streamed forth from the brass ball N for a short time.
18	SW	29.90	59	small	pos.	—	Six o'clock A.M. One o'clock P.M. a low black sky, by which the charge in the rod became negative, with sparks; after continuing so one hour, the electricity changed again to positive. The rod has been in charge 16 hours this day; how much more I cannot say.
19	S	29.20	65	strong	—	neg.	A.M. At four o'clock P.M. fell a mild rain, void of the electric fluid. At five o'clock the rain fell more copiously, and it was strongly charged with negative electricity.
20	S	29.14	64	small	—	neg.	Early this morning I found the humidity of the air electrified negatively. At ten o'clock, the electricity became more intense, so as to emit small sparks: in half an hour's time there were four beautiful gradual changes of electricity. Afterwards the rod remained negative two hours. Three o'clock P.M. the electricity changed, and continued near three hours positive. After this a low black cloud passed over the rod, and let fall a few drops of rain mixed with

Days.	Wind.	Barom.	Ther.	Sparks.	Pos.	Neg.	
		Inches.	°				hail; the rod was now highly charged positively, the bell rang briskly, but soon stopped, while the electricity changed to a strong negative; then it began to ring again, for about ten minutes, after which all signs of electricity ceased in the rod for a few minutes. Afterwards the rod became rather strongly charged for two hours, and changed once in that time. During this high charge of the rod, the bell (which is connected by metal with the moist ground) was in a contrary state of electricity to that of the insulated part of the apparatus; and also the air in the room, the window-shutter nearest the bell, and even the bricks in the wall, were all electrified sufficiently to indicate the kind.
May 21	SW	29.98	58	strong	pos.	—	A.M. by the night bottle, which had been charged while I slept. Four o'clock P.M. a low cloud approached the rod, and dropped a little rain, by which it became electrified strongly negative. This electrification of the rod lasted near four hours, and changed three times in kind.
22	E	29.10	56	0	pos.	—	A.M. One o'clock P.M. wind S, a small rain fell, and the air being very moist, has lessened considerably the insulation of the rod.
23	S	29.90	56	0	pos.	—	The air is dry to-day, but the clouds are very high.
24	NE	29.10	69	strong	—	neg.	Six o'clock A.M. Nine o'clock some rain fell, which increased the negative charge in the rod. The rain now falls more copiously, and the balls close and open gradually with positive electricity, which was rather strong for a short time. The balls again gradually close and open, without change of kind. The rod has been electrified near 14 hours this day. In the morning of to-day, at Lewes in Sussex, much lightning and thunder.
25	NE	29. 4	70	0	—	neg.	A.M. A moist air, and wet morning.
26	E	29.95	62	strong	pos.	—	A dark cloudy morning. Three o'clock P.M. on the approach of a heavy dark cloud, a few drops of rain fell, by which

Days.	Wind.	Barom.	Ther.	Sparks.	Pos.	Neg.	
		Inches.	°				the rod was electrified positively; the pith balls soon opened full four inches, and by and by the bell began to ring slowly. Two gentlemen fortunately being present, and much delighted with the sight, by my desire joined hands with me to make the circuit between the bell and brass ball N, and on taking a spark at the brass ball, we all received a smart shock through our arms and breasts. The wind is now S. with a heavy fall of rain; the bell (as usual) now ceases to ring while the electricity changes to negative: then rings again briskly. The electricity gradually changed twice after this, and ended negative. These several phænomena took up one hour.
May 27	N	29.98	69	0	—	neg.	A.M. A small rain. In the afternoon positive. Near midnight London was visited by vivid darts of lightning, and repeated explosions of thunder, extremely awful and terrific at that time of night. I was in bed, and awaked out of sleep by it.
28	NE	30.	61	small	—	neg.	By the night bottle. A heavy thick air. Seven o'clock P.M. positive with small sparks. Nine o'clock, fell a heavy shower of rain; its electricity was negative.
29	SE	30.	69	0	pos.	—	A.M. and P.M.
30	SW	30. 6	63	strong	pos.	—	At seven o'clock A.M. the weather showery, and the electricity rather strong, but often changing in kind. Seven o'clock P.M. one uniform black cloud darkens the air (yet the sun shines in the west); by and by a heavy rain fell, by which the rod became very strongly electrified positively, the bell rings briskly, and the pith balls are agitated with sudden jerks; the rain abates, and the electricity becomes negative. These operations of nature lasted only 34 minutes; during that time, the electricity was four times positive, and five times negative; and these several changes were all gradual.
31	N	30.	58	strong	—	neg.	The weather, and the electricity of the atmosphere have been this day nearly the same as on the preceding one.

Days.	Wind.	Barom.	Ther.	Sparks.	Pos.	Neg.	
		Inches.	°				
June 1	S	30.12	58	0	pos.	—	
2	N	30.16	69	0	pos.	—	
3	SW	30.12	66	0	pos.	—	Serene fine weather.
4	N	30. 8	61	0	pos.	—	
5	N	30.18	58	0	pos.	—	
6	N	30.16	62	0	pos.	—	A.M. Two o'clock P.M. the sky still clear; the electricity of the rod was now negative, and continued so two hours. During this time, the rod was only electrified with its own electricity, or what has been termed influentially electrified; for I found by trying both extremities of the rod, that they were in contrary states of electricity.
7	W	30.	62	0	pos.	—	A.M. A warm moist air.
8	SW	29.91	66	0	pos.	—	A.M. Some mizzling rain.
9	S	29.65	64	0	pos.	—	A.M.
10	SW	26.60	65	strong	pos.	—	A cloudy morning. Half after one o'clock P.M. the electric charge in the rod became negative. In a short time after, some rain fell, and the electricity again became positive, and very strong; the bell now rang slowly, and soon stopped; the pith balls closed and opened negative, and continued so a full hour: the balls exhibited one sudden jerk during that time. This charge of the rod lasted two hours. During the passage of a cloud over the rod, in the morning of to-day, I found it influentially electrified.
11	N	29.90	65	0	pos.	—	
12	N	30.	59	0	pos.	—	
13	NE	30.29	60	0	pos.	—	The weather serene and clear. The electricity varying between nothing and the balls seven-tenths of an inch open.
14	NE	30.30	64	0	pos.	—	
15	NE	30.30	69	0	pos.	—	
16	N	30.23	54	0	pos.	—	
17	E	30.10	61	strong	—	neg.	After a course of clear serene weather, this day has been dark, and hazy. Nine o'clock P.M. wind S, a prodigious large cloud approached the rod, and a little rain fell, yet there were no signs of electricity in the rod. In a few minutes after this, I was surprised by hearing the bell ring; I ran to the apparatus, and found it highly charged negatively. When the main bulk of the cloud arrived over the rod, its electricity changed to a strong positive;

Days.	Wind.	Barom.	Ther.	Sparks.	Pos.	Neg.	
		Inches.	°				and in a quarter of an hour, the rain and electricity all ceased together. This day, in the evening, at Wolverhampton, fell a heavy rain, accompanied with lightning and thunder.
June 18	NW	29.91	63	0	pos.	—	A.M. and P.M.
19	S	30.	68	0	—	neg.	A dark cloudy morning. The electricity positive at five o'clock P.M.
20	SW	30.21	74	0	pos.	—	Weather very hot, and a weak electricity.
21	SW	30.35	76	0	pos.	—	
22	E	30.20	79	0	pos.	—	A.M. Serene hot weather. Near sunset the wind W, when heavy black clouds were forming, their upper border about 25 degrees above the horizon. The lightning flashed among these clouds from nine to eleven o'clock. But there being neither clouds nor low vapour near the rod, it was not affected thereby. In the evening of this day at Salisbury, a very severe storm of hail, rain, lightning, and thunder.
23	SW	30.	78	0	pos.	—	A.M.
24	W	30.	68	0	pos.	—	A.M. The sky has been overcast all day, and two or three times fell a little rain, driven by a strong wind, but nearly void of electricity.
25	W	29.90	68	strong	pos.	—	At six o'clock A.M. Near eleven o'clock, a shower of rain fell, strongly negative. At the same time, in a *lower stratum* of air, the hand exploring rod was electrified *positively*. Abundance of small white clouds passed over the rod, which kept the pith balls continually closing and opening, with negative electricity. Eight o'clock P.M. some heavy drops of rain fell; the rod now became very strongly electrified; the bell rang slowly, and soon stopped, while the electricity changed to a strong positive, then rang again more briskly for a longer time. After this there were two more gradual changes.
26	NW	29.90	63	strong	pos.	—	The weather, and atmospheric electricity, have been this day nearly the same as on the preceding one; only the operations in the rod have been more powerful. For in one charge of the rod today, I counted about forty dense sparks, which spontaneously struck between

Days.	Wind.	Barom.	Ther.	Sparks.	Pos.	Neg.	
		Inches.	°				the bell and brass ball N in quick succession. These sparks were about two inches long.
June 27	N	29.96	61	strong	pos.	—	A.M. Four o'clock P.M. I saw heavy clouds forming; and as they came nearer and nearer, the rod became more powerfully electrified with negative electricity. It now began to rain, by which the rod became intensely electrified indeed. Many dense sparks now struck through the air, between the bell and brass ball N, with a loud report for so small an opening of two inches only. There were six changes of electricity in two hours.
28	W	30. 5	57	0	pos.	—	Mild foggy weather; moderately electrified.
29	N	30.	70	0	pos.	—	
30	SW	30.	67	0	pos.	—	
July 1	W	29.83	66	small	pos.	—	A.M. Negative P.M. with small sparks.
2	W	29.77	63	strong	pos.	—	A.M. Half after twelve o'clock P.M. a sudden gust of wind sprung up, followed by a shower of rain, which electrified the rod positively, and sufficient to emit small sparks; which soon afterwards became negative. Near four o'clock P.M. a very large black cloud passed over the rod, and let fall a little rain mixed with hail, by which the rod became highly electrified negatively: the bell now rang briskly, till a flash of lightning and instant crack of thunder happened, which occasioned a sudden change in the electricity to positive: then the bell struck up again as brisk as before. The rod has been charged ten hours without intermission this day.
3	NW	29.72	70	0	pos.	—	A.M. A cloudy atmosphere, but fair.
4	SW	29.66	69	0	pos.	—	A.M. Dull heavy weather.
5	SW	29.40	64	strong	—	neg.	This day has been showery and sunshine by turns. The rod has been rather strongly electrified, and there have been four gradual changes of electricity. The bell rang briskly a long time. This charge lasted full five hours.
6	N	29.70	65	0	—	neg.	A strong gale of wind with rain, and a weak electricity.
7	N	30.10	63	0	—	neg.	
8	SW	30.	63	0	pos.	—	A.M.
9	SW	29.86	67	0	—	neg.	A.M. A thick moist air.
10	SW	29.90	61	strong	pos.	—	A.M. Near five o'clock P.M. there was

Days.	Wind.	Barom.	Ther.	Sparks.	Pos.	Neg.	
		Inches.	°				a fine shower of rain, by which the rod first became electrified negatively, and afterwards positively, with sparks. This charge lasted five hours.
July 11	NW	29.60	61	strong	—	neg.	Half after ten o'clock A.M. large dark clouds frequently passed over the rod, by which it became electrified negatively. When they passed somewhat nearer, and dismissed some large drops of rain, the rod then became highly charged, and the bell rang briskly, and continued its ringing near half an hour. The pith balls were very much agitated, sometimes with a pendulous motion, then started closer or wider asunder several times. But the most extraordinary phænomenon of to-day is, that the rod continued ten hours electrified negatively, without change of kind: during that time the pith balls never closed.
12	N	29.70	63	strong	—	neg.	The operations in the rod to-day and yesterday, when compared, appear to have but little resemblance. This day the electricity has changed four times, and the bell rang briskly for a minute or two between each change of electricity.
13	W	29.62	64	strong	pos.	—	Two o'clock P.M. the wind S. A sudden shower of rain fell, by which the rod became electrified positively, but soon changed to negative. The bell now rang weakly one minute, and then stopped while the electricity changed to positive, which soon changed again to negative. The electricity and the weather have been much varied to-day. At New-Mills, in Scotland, some lightning and thunder.
14	NW	29.57	57	strong	—	neg.	The weather this day has been rather more varied than yesterday, with a continued succession of showers and sunshine: and the atmospheric electricity has been considerably more intense, and constant. There have been five gradual changes of electricity; between four of those changes, the bell rang briskly. The pith balls also exhibited some of their curious agitations; one jerk in

Days.	Wind.	Barom.	Ther.	Sparks.	Pos.	Neg.	
		Inches.	°				particular was so very quick and powerful, that the balls were near striking each other. In a few minutes after this, I heard distant thunder. At Burwash, in Sussex, this day a heavy tempest of rain, lightning, and thunder.
July 15	NW	29.78	60	0	pos.	—	
16	NW	29.95	63	0	pos.	—	
17	SW	30.13	69	0	pos.	—	
18	W	30.10	66	0	pos.	—	
19	SW	30. 5	65	0	pos.	—	
20	SW	29.75	66	0	pos.	—	
21	W	29.78	63	0	pos.	—	
22	NW	30. 5	61	0	pos.	—	The weather these 19 days has been generally moderate; and the electricity of the atmosphere has been only what is common for serene weather, at no time sufficiently strong to afford visible sparks.
23	SW	30.	64	0	pos.	—	
24	W	30.	66	0	pos.	—	
25	SW	30.12	69	0	pos.	—	
26	SW	30.15	66	0	pos.	—	
27	W	30.11	67	0	pos.	—	
28	S	30.10	63	0	pos.	—	
29	N	29.65	57	0	pos.	—	
30	NW	29.65	60	0	pos.	—	
31	SW	29.55	63	0	pos.	—	
Aug. 1	NW	29.92	59	0	pos.	—	
2	SW	29.95	61	0	pos.	—	
3	SW	29.77	63	strong	pos.	—	A heavy cloudy morning, but fair. Half after one o'clock P.M. a smart shower of rain fell, by which the rod became highly charged with negative electricity; when the cloud was fully over the rod, the electricity changed to positive; and when it had passed the zenith, the electricity changed again to negative.
4	NW	29.83	62	0	pos.	—	
5	SW	30.	67	0	pos.	—	Serene weather, and a weak electricity.
6	SW	29.93	67	0	pos.	—	
7	SW	30.	72	0	pos.	—	
8	W	29.92	67	0	pos.	—	A heavy cloudy morning, but fair. The electricity of the atmosphere has been so weak to-day, that none could be obtained till after sunset.
9	W	30.	63	0	pos.	—	A.M.
10	SW	30. 5	63	0	pos.	—	
11	W	30.10	66	0	pos.	—	
12	SW	30.10	72	0	pos.	—	Serene weather: the pith balls varying between nothing and three-quarters of an inch open.
13	SW	30. 5	69	0	pos.	—	
14	N	30.10	64	0	pos.	—	
15	S	30. 3	72	0	pos.	—	
16	SW	29.90	73	0	pos.	—	There have been several small showers of

Days.	Wind.	Barom.	Ther.	Sparks.	Pos.	Neg.	
		Inches.	°				rain this day, some of them not sensibly electrified, others were weakly positive, but which I could not distinguish from the common electricity of serene weather, which has prevailed a long time.
Aug. 17	W	30.	66	0	pos.	—	Serene weather; divergency of the pith balls from one to seven-tenths of an inch.
18	N	30.10	66	0	pos.	—	
19	SW	30. 1	66	0	pos.	—	
20	NW	30. 4	76	0	pos.	—	
21	S	29.75	77	strong	pos.	—	A.M. Five o'clock P.M. fell some large drops of rain, by which the rod was highly charged positively; the bell rang briskly about five minutes, then stopped, and the electricity of the rod became negative. The rain now ceased, the sky cleared up, and the electric charge in the rod decreased considerably, and ended as it began, positive.
22	W	30. 4	62	0	pos.	—	Dark cloudy weather, and a very moist air, attended with a weak electricity.
23	SW	29.95	67	0	—	neg.	
24	W	30.	63	0	pos.	—	
25	SW	29.96	62	0	pos.	—	
26	N	29.70	64	0	pos.	—	
27	N	29.83	62	strong	pos.	—	This day has been very showery, yet only one shower was strongly electrified.
28	W	29.95	59	0	pos.	—	Serene weather, and a constant electricity.
29	NW	30.	60	0	pos.	—	
30	W	30.12	62	0	pos.	—	
31	SW	30.10	66	0	pos.	—	
Sept. 1	W	29.95	67	0	pos.	—	
2	SW	29.80	62	small	pos.	—	A.M. In the afternoon, there was a thick dark sky, with small rain, and a strong wind. On an increase of rain the electricity became negative, with small sparks. When the rain abated, the electricity changed to positive, and so it ended.
3	W	29.50	58	strong	pos.	—	A.M. Near one o'clock P.M. the electric charge in the rod was negative. I then saw thunder-clouds forming in the south, and in the north. Some large drops of rain now fell, by which the charge in the rod became very strong; the bell now rings faintly, and the pith balls exhibit their waving motion, and sometimes sudden jerks. The distinct thunder-cloud, which was in the north, is now gone by the west into the south, during which the charge in the rod be-

Days.	Wind.	Barom.	Ther.	Sparks.	Pos.	Neg.	
		Inches.	°				came weaker and weaker, till it ended as it began, negative. This electric charge of the rod continued near one hour and a half.
Sept. 4	W	29.65	58	strong	pos.	—	A.M. One o'clock P.M. wind NW. Thin white clouds have been passing over the rod; but a distinct black cloud is now approaching, and some rain falls, mixed with hail; the rod is now highly charged with positive electricity; the bell rang loud, and a long time; and as the shower abated, so did the strength of the electric charge abate in the rod. This charge lasted near three hours, without change of kind.
5	NW	29.70	55	small	pos.	—	A quarter before three o'clock P.M. some rain fell, by which the rod became moderately charged positively, but soon changed to a weaker negative. There were two more gradual changes of electricity within the space of one hour.
6	N	30.	58	strong	—	neg.	Three o'clock P.M. a large dark cloud passed over the rod with drizzling rain; its electricity was negative; the bell rang weakly. The electric charge from this cloud began, continued, and ended, negative.
7	N	30.10	51	small	pos.	—	A.M. by the night bottle. Two o'clock P.M. a dark cloud, with drizzling rain, passed over the rod, by which it was electrified positively, with sparks; and ended negative.
8	N	30. 5	57	0	pos.	—	Moderate fine weather, and a weak electricity.
9	SW	30. 8	62	0	pos.	—	
10	NW	30.	58	0	pos.	—	
11	SW	30. 1	51	0	pos.	—	
12	SW	30.10	62	0	pos.	—	
13	W	30.	61	0	pos.	—	
14	S	29.84	62	0	pos.	—	
15	SW	29.80	66	small	pos.	—	The rod has been electrified positively nearly all this day. At five o'clock P.M. an awful darkness covered the heavens, until near six o'clock, succeeded by a drizzling rain, from which I could not obtain (in my common way) any electric fluid: but when the dark cloud became broken, and no rain fell, I then obtained visible sparks at the ball N, which continued until I went to bed;

Days.	Wind.	Barom.	Ther.	Sparks.	Pos.	Neg.	
		Inches.	°				and seemed to indicate that it would remain so all night, as the night is perfectly serene, and clear moonlight.
Sept. 16	W	30.	55	small	pos.	—	Five o'clock this morning, I found the rod charged positive, but not quite so strong as when I left it last night; and it gradually went off by nine o'clock, and returned again into the rod at near four o'clock P.M. from which time the fluid gradually increased with the cool of the evening, until visible sparks appeared at the brass ball N. In this state of intensity I left it at near eleven o'clock.
17	W	30.28	50	small	pos.	—	This morning, a little after five o'clock, I found the rod rather stronger charged with the same electricity than when I left it last night; and I have no doubt but it has been electrified all night. The pith balls have never closed this day: their divergency has been between half an inch and two inches. Weather serene.
18	SW	30.10	54	small	pos.	—	Five o'clock A.M. I found the rod electrified positively, and nearly of the same intensity it was in when I left it last night; that is to say, capable of emitting small sparks. The atmosphere this morning is perfectly serene. There is a dew, or low fog, about five or six feet high, spread over the ground in Hyde-Park, very beautiful to look upon from an upper window. For the space of two hours, this vapour gradually increased, and ascended higher and higher, until it quite intercepted the trees from my sight, and became a pale wet fog; which very much injured the insulation of the rod: for during its increase in height, the electricity of the rod decreased, insomuch, that I had almost concluded that the pith balls touched each other. But during this short suspense the sun rose higher, and diminished the fog very fast; and the electricity in the rod soon increased to its former strength; and so continued all day.
19	S	29.85	63	small	pos.	—	Five o'clock A.M. I found the rod electrified positively, but rather weaker than

Days.	Wind.	Barom.	Ther.	Sparks.	Pos.	Neg.	
		Inches.	°				when I left it last night. The weather this day has been nearly the same with that of the preceding one; only the fog began later, and continued longer. The rod has been electrified all this day.
Sept. 20	W	29.70	57	strong	pos.	—	Near six o'clock A.M. I found the rod electrified, but very weak, barely sufficient to indicate the kind, and it remained so till near three o'clock P.M. The pith balls now closed (which I suppose has not happened before during the space of 92 hours) and opened negative. A heavy cloud from the NNW now approaches the rod, and a little rain falls, by which the rod is highly electrified negatively, and the bell rings briskly; the pith balls diverge to their utmost limit, and exhibit their waving pendulous motion. These operations took up about one hour; and the rod continued electrified negatively the rest of the day.
21	NW	29.90	58	0	—	neg.	By the night bottle. The electricity in the rod barely sensible.
22	W	30.20	52	small	pos.	—	The pith balls have not closed this day. The weather serene.
23	S	29.95	52	small	pos.	—	This morning a quarter before six o'clock, I found the electric charge in the rod much weaker than when I left it last night; which I afterwards found had been occasioned by a spider fixing its web from the rod to the wall of the house; for when the web was removed, the intensity of the electrical charge was soon increased.
24	N	30.30	58	0	pos.	—	} Dark cloudy weather, and a strong wind.
25	NW	30.30	58	0	pos.	—	
26	NW	30.38	55	small	pos.	—	Between six and seven o'clock this morning, I found the pith balls quite closed; nor would they even stick to my finger when I touched them. At this instant there was a visible dew spread over the ground in Hyde-Park: I examined it with my hand exploring-rod, and quickly charged an electrometer sufficient to ascertain the kind, which was positive. I now suspected my daily enemies the spiders; I therefore went upon the roof of the house with my

Days.	Wind.	Barom.	Ther.	Sparks.	Pos.	Neg.	
		Inches.	°				long feather broom, and gently swept the rod down; on examining the broom, I found a spider's web entangled in the feathers. I then went down to the apparatus, and found the pith balls a quarter of an inch open positive, which gradually increased, even to visible sparks.
Sept. 27	SW	30.35	57	small	pos.	—	Six o'clock A.M. I found the electric charge in the rod much weaker than when I left it last night. Nevertheless, when the sun got up a little above the horizon, the rod became stronger electrified. The pith balls never closed this day.
28	SE	30.12	48	small	pos.	—	The operations of the rod to-day have been nearly the same with the preceding one.
29	N	30.12	55	small	pos.	—	This morning a little before six o'clock, I found the pith balls closed. Atmospheric electricity has been more variable this day than for many days past; sometimes quite null, then weak, or strong by turns, until three o'clock P.M. when it again became constant during the remainder of the day.
30	NE	30.25	58	small	pos.	—	Six o'clock A.M. I found the pith balls open full three quarters of an inch positive, and they continued open all day. Their least divergency (as usual) about midday. The weather still thick and dark in the forenoon; and clear serene weather in the afternoon.
Oct. 1	E	30. 5	52	small	pos.	—	Six o'clock A.M. I found the pith balls opened near one inch and a quarter; and they never closed this day. Their least divergency, as usual, was about midday, and their greatest was near midnight. Weather serene.
2	E	29.84	56	small	pos.	—	Six o'clock A.M. I found the pith balls nearly closed, and the state of the air moist and foggy. Half after seven o'clock, the balls opened a little wider, and continued to increase in strength, so that between eleven and twelve o'clock their divergency was full three inches. I saw nothing in particular to occasion it; and I thought it was above the intensity of common serene weather. I saw indeed a dark cloud, but it was

Days.	Wind.	Barom.	Ther.	Sparks.	Pos.	Neg.	
		Inches.	°				at a very great height. However, this electric charge continued in the rod until seven o'clock P.M. when a shower of rain fell, and the pith balls now closed, and opened negative: a fact which has not happened for eight days. After the rain had ceased, the electricity became again positive.
Oct. 3	SE	29.80	63	0	pos.	—	A wet morning, the air moist, and a weak electricity.
4	NE	29.90	56	small	pos.	—	A very moist air. Half after ten o'clock, fell a heavy rain, which continued three hours. Its electricity was positive, but not strong.
5	SW	30. 2	58	small	pos.	—	A foggy damp morning. The rod has been electrified very near all this day.
6	S	29.85	60	strong	—	neg.	A heavy cloudy morning. Two o'clock P.M. the clouds float in the air lower than in the morning; and the rod is electrified negatively. Four o'clock, a heavy black cloud passed the rod, by which it was strongly electrified, but still negative, and continued so a full hour. During this time there were several sudden jerks exhibited by the pith balls. Near sunset the balls closed, and opened positive, and remained so. In the east part of Kent, some lightning and thunder this day.
7	S	29.85	61	0	pos.	—	A.M. the weather mild and fair.
8	N	29.85	54	small	—	neg.	By the night bottle: by the rod positive.
9	N	29.90	52	0	pos.	—	A.M. Perfectly fine weather.
10	W	30. 4	42	0	pos.	—	The electricity constant this day.
11	SW	30. 8	39	small	pos.	—	The rod electrified all this day.
12	SW	29.86	56	small	pos.	—	A little after six o'clock A.M. I found the rod electrified, but rather weaker than when I left it last night. The weather cloudy, with a thick moist air. One o'clock P.M. fell a sudden shower of rain; the electricity now changed to negative, with small sparks.
13	W	29.75	50	small	pos.	—	A little after six o'clock A.M. I found the rod electrified positively. A warm mizzling rain and moist air. Ten o'clock P.M. balls open two inches.
14	W	29.80	61	0	pos.	—	Six o'clock A.M. balls open half an inch positive.
15	W	30.	59	small	pos.	—	A.M. Fair weather, and strong gales of wind.

Days.	Wind.	Barom.	Ther.	Sparks.	Pos.	Neg.	
		Inches.	°				
Oct. 16	W	30.34	43	small	pos.	—	Six o'clock A.M. balls three-tenths of an inch open. The pith balls have not been unelectrified this day.
17	S	30.15	55	small	pos.	—	During a continuance of moderate weather, I have frequently observed that the atmospherical electricity has periods of increase and decrease of quantity, which are tolerably regular; I shall therefore be accurate in my account of this day's variation, as it may serve for an example of reference to many other days when similar appearances took place. When I left the rod last night, the pith balls were one inch open, positive. Six o'clock this morning, I found them three-tenths of an inch open, positive; and soon after this I perceived them to open wider and wider, so that a little before nine o'clock, the pith balls were stationary at near two inches open. But as the sun advanced towards the meridian, the electric charge in the rod decreased; and it was only possessed of various degrees of a weak intensity until four o'clock P.M.: the pith balls were then only two-tenths of an inch open. Half an hour after four o'clock, they began to increase their divergency, which gradually held on till a little after seven o'clock, when they again became stationary at full two inches open; a little after eight o'clock, they began to decrease; nine o'clock, they were one inch and a quarter open; near eleven o'clock, I left them at a bare inch open.
18	SW	29.90	62	small	pos.	—	Six o'clock, pith balls two-tenths of an inch open, positive. A dark cloudy morning. One o'clock P.M. some rain fell, yet the electric charge in the rod was positive. Near three o'clock, the fall of rain suddenly increased, so did the electric charge in the rod; and at the same time changed to negative, with sparks; and after this ended positive.
19	W	30.15	39	small	pos.	—	The rod has not been unelectrified this day: and the facts and appearances have been nearly like those of the 17th instant.

Days.	Wind.	Barom.	Ther.	Sparks.	Pos.	Neg.	
		Inches.	°				
Oct. 20	E	30. 5	46	small	pos.	—	The operations of the electricity in the rod to-day have been nearly the same with those of the 17th instant.
21	SE	29.91	62	small	pos.	—	The electric charge in the rod has been constant this day; and for particulars may justly be referred to the 17th instant.
22	SE	29.78	59	small	pos.	—	A.M. The pith balls closed and opened, without change of kind, several times this day: and they have been very irregular when compared with many of the preceding. A thin wet fog has prevailed all this day.
23	SE	29.75	53	small	pos.	—	A.M. Four o'clock P.M. fell a mizzling warm rain, by which the rod became electrified negatively, and emitted small sparks. When the rain was over the rod was electrified positively, and it continued so.
24	NE	29.76	54	small	pos.	—	The rod has been constantly electrified this day, and has been strictly periodical. See 17th instant.
25	N	29.75	54	small	pos.	—	The same as the preceding day.
26	N	29.89	50	small	pos.	—	The operations of the electric fluid in the rod this day have been nearly similar to those of the 17th instant.
27	E	29.90	49	0	pos.	—	Dark moist air, but fair, and some brisk gales of wind.
28	NE	29.73	49	0	pos.	—	
29	NE	29.80	47	0	pos.	—	
30	NE	29.85	44	0	pos.	—	
31	N	29.88	42	small	pos.	—	The thick easterly wind is now over-ruled by a clear northerly one: and the electrification of the rod this day has been of the periodical kind. The pith balls never closed.
Nov. 1	S	29.80	46	strong	pos.	—	Near six o'clock A.M. I found the pith balls open one inch and a quarter, positive; which is rather less than when I left them last night. Half an hour after ten o'clock, fell a heavy shower of rain: the pith balls now closed, and soon opened negative, which, with the rain, continued for the rest of the day.
2	SW	29.45	49	0	pos.	—	P.M. fair weather.
3	SW	29.80	44	strong	pos.	—	From a continued rain of four hours.
4	W	29.74	49	small	pos.	—	Nearly all day, but strongest after sunset.
5	W	29.95	47	0	pos.	—	A.M. and P.M. A cloudy atmosphere.
6	S	29.75	53	0	pos.	—	The electricity of the atmosphere this

Days.	Wind.	Barom.	Ther.	Sparks.	Pos.	Neg.	
		Inches.	°				day has been so weak, that I have been obliged to content myself with obtaining the kind by a pair of flaxen threads, without balls to them. The weather warm and showery.
Nov. 7	S	29.68	57	small	pos.	—	The operations in the rod, these two
8	NE	29.90	42	small	pos.	—	days, have been of the periodical kind. See 17th of October.
9	NE	30. 3	44	o	pos.	—	Pith balls not open this day. The kind was obtained by flaxen threads without balls to them. A moist air.
10	NE	30.15	46	o	pos.	—	
11	E	30.	48	o	pos.	—	
12	NE	30.10	47	o	pos.	—	
13	NE	30.15	44	o	pos.	—	
14	NE	30.25	45	o	pos.	—	Sharp dark weather, with a dry easterly wind; and a weak electricity.
15	NE	30.29	42	o	pos.	—	
16	NE	30.16	38	o	pos.	—	
17	E	30.	41	o	pos.	—	
18	N	29.77	33	o	pos.	—	
19	S	29.24	47	strong	—	neg.	The severe easterly wind is now gone, and a more intense atmospheric electricity is returned. Nine o'clock A.M. a large black cloud passed over the rod, and a moderate shower of rain fell, by which the rod was rather strongly electrified negatively. This shower lasted one hour; and near the middle of it, the electricity changed to a strong positive; after this, the electricity ended as it had begun, negative.
20	S	29.25	42	small	—	neg.	By the night bottle. Half an hour after two o'clock P.M. fell some rain, and the rod became electrified negatively. At Calstock, in Cornwall, in the evening of this day, much lightning and thunder.
21	W	29.16	47	strong	—	neg.	Half an hour after midday, fell a moderate shower of rain, attended with a great darkness. The rod now became rather strongly electrified negatively; and the bell rang weakly. There were after this several showers, which only gave a weak positive electricity; except one, which fell at ten o'clock at night; this indeed was strongly positive. At eleven o'clock this night, both at London and Portsmouth, there was much lightning and thunder. And also, at the

Days.	Wind.	Barom.	Ther.	Sparks.	Pos.	Neg.	
		Inches.	°				same time, at Salisbury, where it appeared to roll along the ground like a body of liquid fire.
Nov. 22	SW	29.40	49	0	—	neg.	A.M. The weather showery.
23	NW	29.68	49	0	pos.	—	P.M. A foggy moist air.
24	SW	29.84	37	0	pos.	—	A.M. and P.M.
25	S	29.78	50	0	—	neg.	P.M. Weather showery.
26	NW	29.58	51	0	pos.	—	Atmospheric electricity, these four days, has been governed by its periodical laws. The weather very mild.
27	N	30.28	36	0	pos.	—	
28	N	30.35	39	small	pos.	—	
29	SE	30.12	35	small	pos.	—	
30	N	29.74	32	small	pos.	—	From a shower of snow.
Dec. 1	SE	29.62	32	0	pos.	—	There has been very moderate weather these 12 days. The pith balls have been variable, between null, and one inch open.
2	W	29.42	44	0	pos.	—	
3	S	29.83	45	0	pos.	—	
4	W	29.95	42	0	pos.	—	
5	NW	29.95	32	0	pos.	—	
6	N	30.30	38	0	pos.	—	
7	W	30.30	38	0	pos.	—	
8	N	30.15	49	0	pos.	—	
9	N	30.15	48	0	pos.	—	
10	W	30.18	48	0	pos.	—	
11	SW	29.95	52	0	pos.	—	
12	SW	30. 7	37	0	pos.	—	
13	SW	29.88	48	strong	—	neg.	There has been a constant drizzling rain, with a strong gale of wind, from early in the morning to seven o'clock P.M.; the wind then became a storm. The rod had been null to this time, but now gave signs of a negative charge, and continued increasing its charge until near nine o'clock; at this time the storm seemed to be at its height. The wind now suddenly shifted from SW to N: every appearance is now awful; the pith balls diverge to their utmost limits, and are very much agitated; and the bell rang for half a minute. Half an hour after nine o'clock, the storm ceased; and the charge in the rod became very weak. During the space of two hours the electricity changed six times.
14	W	29.91	39	0	pos.	—	A.M.
15	W	29.40	48	small	—	neg.	From five to eight o'clock A.M. there was a storm of wind and rain. The electricity changed once only.
16	S	29.71	39	0	—	neg.	P.M. A showery day.
17	SW	29.41	41	0	pos.	—	A.M.

Days.	Wind.	Barom.	Ther.	Sparks.	Pos.	Neg.	
		Inches.	°				
Dec. 18	N	28.98	37	small	—	neg.	A.M. and P.M. Strong gales of wind, with rain.
19	S	29.54	33	strong	pos.	—	Between eleven and twelve o'clock A.M. fell a shower of rain, mixed with snow, by which the rod became rather strongly electrified, positively; which continued, with some variation of intensity (without change of kind) near three hours. The greatest divergency of the pith balls was four inches, when fine sparks were received from the brass ball N.
20	N	29.93	32	0	pos.	—	Nearly all day. Divergency three-quarters of an inch. Weather serene.
21	SW	29.90	46	0	pos.	—	The air is extremely moist; and some small rain fell several times this day, which was almost void of the electric fluid; for I could not obtain it in quantity sufficient to ascertain the kind, until I added a *lighted pitch torch* to my hand exploring-rod.
22	W	30. 5	37	0	pos.	—	The pith balls half an inch open nearly all day.
23	W	29.80	44	0	pos.	—	Five o'clock this morning, a most tremendous storm of wind, rain, lightning, and thunder, visited London and other parts. I, being not well, and in bed, did not get up to make observations. I thought I heard the bell ring: however, the rod was but weakly electrified the rest of the day. The high wind continued, without rain, all day.
24	SW	30.20	39	0	pos.	—	Moderate weather, and nearly a constant charge of electricity in the rod.
25	N	30.	45	0	pos.	—	
26	SW	30.22	29	small	pos.	—	
27	S	30. 3	31	0	pos.	—	
28	N	30.	32	0	pos.	—	
29	S	30.20	25	0	pos.	—	
30	S	29.96	37	small	—	neg.	P.M. A fair morning, and a wet evening.
31	N	30. 5	38	0	pos.	—	The rod has been electrified nearly all day, by a fog.
Jan. 1 1791.	S	30. 2	40	0	pos.	—	A.M. In the evening negative, from a fall of rain. This evening much lightning and thunder at Canterbury.
2	SW	29.72	31	small	pos.	—	A.M. A foggy morning. Half an hour after eight o'clock P.M. happened a sudden gale of wind and rain, by which the rod became rather strongly electrified, negatively.

Days.	Wind.	Barom.	Ther.	Sparks.	Pos.	Neg.	
		Inches.	°				
Jan. 3	SW	29.40	29	small	pos.	—	The morning of this day was frosty, and the evening wet. The electricity of the former was positive, and the latter negative.
4	SW	29.40	38	0	—	neg.	P.M. Balls three-quarters of an inch open. Strong gales of wind, with rain. Nine o'clock P.M. severe lightning and thunder at Falmouth.
5	SW	29.29	35	0	pos.	—	Very fine weather. Divergency of the pith balls has been between null and near one inch open.
6	SW	29.46	37	0	pos.	—	
7	W	29.18	51	0	pos.	—	
8	W	29.26	43	0	pos.	—	
9	W	28.96	42	small	—	neg.	Eight o'clock A.M. I saw a large black cloud in the N and NW: soon after, a strong wind suddenly sprang up, and a little rain fell; during which, the rod was rather strongly electrified negatively, with small sparks. This charge of the rod only lasted eighteen minutes.
10	S	29.75	42	0	pos.	—	Fine weather, and almost a constant electricity.
11	W	29.75	48	0	pos.	—	
12	S	29.95	42	0	pos.	—	P.M. A very high wind, and a weak electricity. In the evening of this day, much lightning and thunder at Plymouth.
13	W	29.48	43	0	—	neg.	P.M. from a shower of rain.
14	W	29.80	42	0	pos.	—	Very moderate weather, and a weak electricity.
15	SW	30.	32	0	pos.	—	
16	SW	29.82	50	0	pos.	—	
17	SW	29.50	49	0	pos.	—	
18	S	29.50	42	0	—	neg.	From rain, with a strong wind.
19	S	28.87	43	0	—	neg.	
20	SW	28.48	43	0	pos.	—	The weather for these twelve days having been moderate, the electricity of the atmosphere has only been of such intensity as is common to serene weather.
21	W	29.22	37	0	pos.	—	
22	S	29.85	35	0	pos.	—	
23	W	29.76	48	0	pos.	—	
24	S	30.50	32	0	pos.	—	
25	SW	30.27	45	0	pos.	—	
26	SW	30.15	45	0	pos.	—	
27	SW	29.84	45	0	pos.	—	
28	N	30.	33	0	pos.	—	
29	SW	30.	38	0	pos.	—	
30	NW	29.85	44	0	pos.	—	
31	NW	29.85	51	0	pos.	—	
Feb. 1	W	29.85	47	small	pos.	—	P.M. from a shower of rain, of large drops; the rod became electrified rather strongly positive, and emitted fine bright sparks at the brass ball N.

Days.	Wind.	Barom.	Ther.	Sparks.	Pos.	Neg.	
		Inches.	°				
Feb. 2	N	30.	38	0	pos.	—	The weather these nine days has been foggy and clear by turns; and the electrification of the atmosphere constant.
3	N	30.20	32	0	pos.	—	
4	N	30.40	32	0	pos.	—	
5	N	30.45	36	0	pos.	—	
6	SW	30.40	36	0	pos.	—	
7	NW	30.32	42	0	pos.	—	
8	W	30.36	43	0	pos.	—	
9	SW	30.32	43	0	pos.	—	
10	SW	30. 8	46	0	pos.	—	
11	SW	29.80	49	0	pos.	—	There has been, these three days, a strong wind, with mizzling rain; and the electricity so weak, that I was obliged to connect, with the hand-rod, *a lighted torch*, to ascertain the kind.
12	W	30.11	36	0	pos.	—	
13	SW	29.95	46	0	pos.	—	
14	SW	29.88	53	0	pos.	—	Fine mild weather, with a constant electricity.
15	SW	29.80	47	0	pos.	—	
16	SW	29.85	43	0	pos.	—	
17	SW	30.10	40	0	pos.	—	
18	SW	29.35	30	0	pos.	—	
19	NE	29.40	37	strong	pos.	—	Five o'clock, a dark cloudy morning, with some small rain; by which the rod became rather strongly electrified, positively. During the passage of one low cloud, the electric charge in the rod changed five times; all of which changes, I have reason to think, were only influentially effected.
20	NE	29.80	40	small	pos.	—	A.M. Rainy dark weather. In the afternoon the charge in the rod became negative, and remained so full three hours, without change of kind.
21	S	29.56	42	0	pos.	—	Dark rainy weather, and a very weak electricity.
22	SW	29.73	32	0	pos.	—	
23	SW	29.55	47	0	pos.	—	
24	W	29.90	33	0	pos.	—	
25	E	30.	37	strong	pos.	—	There has happened this day a continued rain, mixed with snow. The rod has been several times strongly charged; and the electricity gradually changed four times.
26	N	29.72	33	strong	pos.	—	The weather and the electrification of the rod have been nearly the same as in the preceding day.
27	N	30.	38	strong	pos.	—	The weather and the operations of the rod have been nearly the same as in the two preceding days.
28	N	30. 8	36	0	pos.	—	The weather has been foggy these three days.
March 1	N	30.12	35	0	pos.	—	
2	S	30.25	32	0	pos.	—	

Days.	Wind.	Barom.	Ther.	Sparks.	Pos.	Neg.	
		Inches.	°				
March 3	SW	30.50	32	0	pos.	—	
4	SW	30.40	45	0	pos.	—	
5	SW	30.44	47	0	pos.	—	
6	SW	30.20	40	0	pos.	—	
7	NW	30.25	41	0	pos.	—	
8	N	30.55	37	0	pos.	—	
9	SW	30.65	34	0	pos.	—	Serene fair weather these seventeen days; and the nights were generally frosty. The electricity of the atmosphere has been constant; but too weak to afford visible sparks.
10	W	30.55	35	0	pos.	—	
11	SW	30.46	39	0	pos.	—	
12	W	30.28	40	0	pos.	—	
13	SW	30.20	44	0	pos.	—	
14	SW	30.18	47	0	pos.	—	
15	SW	30.35	50	0	pos.	—	
16	SW	30.40	51	0	pos.	—	
17	SW	30.40	40	0	pos.	—	
18	SW	30.28	50	0	pos.	—	
19	SW	30.15	39	0	pos.	—	
20	W	29.68	46	0	—	neg.	This morning fell a warm drizzling rain; which seems to have acted like a charm on the late constant electrification of the atmosphere: for I find it so weakly electrified since the rain fell, that I was obliged to connect, with the rod, *a lighted torch*, to ascertain the kind.
21	NW	29.15	44	strong	pos.	—	Four o'clock this morning, a severe storm of wind and rain, driving full against my bed-room window, induced me to rise from my bed to see the state of the rod; which I found highly electrified, positively. This wind did not abate its force till sun-set. Four o'clock P.M. some rain fell, mixed with snow, which lasted near one hour. During this time the rod was most powerfully electrified positively: I counted seventeen spontaneous explosions between the brass ball and bell, notwithstanding the bell was ringing all the time. Half an hour after five o'clock, came a second shower of rain, hail, and snow, which lasted half an hour, attended with uncommon darkness. During about half this time, the electric charge in the rod was as strong as it could possibly be; for the fluid almost streamed between the brass ball and bell: I counted near two hundred very brilliant explosions between the ball and bell. The bell rang all the time. This latter very high

Days.	Wind.	Barom.	Ther.	Sparks.	Pos.	Neg.	
		Inches.	°				charge was negative. There were four changes of electricity, but all gradual.
Mar. 22	N	29.85	44	0	pos.	—	The disposition of the atmosphere has been mild and regular these four days; and the state of atmospheric electricity constant and periodical.
23	W	30. 3	54	0	pos.	—	
24	N	30.50	44	0	pos.	—	
25	S	30.38	43	0	pos.	—	
26	S	29.85	46	small	—	neg.	Half an hour after nine o'clock A.M. fell some rain, by which the rod became electrified negatively. The wind now became N, and attended with showers of rain the rest of the day: all of them moderately electrified, negatively.
27	N	30.	40	0	pos.	—	The weather has been fair during these ten days, and the atmospheric electricity has been constant; yet it has been too weak to afford visible sparks.
28	NW	30.10	45	0	pos.	—	
29	SW	30. 6	50	0	pos.	—	
30	E	30.	44	0	pos.	—	
31	SE	30. 8	44	0	pos.	—	
April 1	SW	30.	50	0	pos.	—	
2	E	29.81	51	0	pos.	—	
3	E	29 78	50	0	pos.	—	
4	NE	29.90	46	0	pos.	—	
5	NE	29.72	46	0	pos	—	
6	N	29.45	43	small	—	neg.	Six o'clock A.M. I found the night bottle charged sufficiently to explode with a visible spark; the kind, negative. During the night, the weather had changed from dry to a mild rain. The rod has been electrified negatively nearly all this day.
7	W	29.78	47	0	pos.	—	Mild serene weather. The divergency of the pith balls has been varying between null and one inch open.
8	NW	29 92	53	0	pos.	—	
9	NW	29.90	51	0	pos.	—	
10	W	30.	50	0	pos.	—	
11	SE	30.	50	small	—	neg.	Weather still mild, and a constant electricity this day.
12	N	29.95	54	0	pos.	—	From a fog.
13	NE	30.	51	0	pos.	—	Mild serene weather these nine days; some of them foggy: but whether clear or foggy, the vapour in the air has been constantly electrified, positively.
14	W	30.	59	0	pos.	—	
15	SW	30. 6	55	0	pos.	—	
16	SE	30.10	55	0	pos.	—	
17	SE	30.	61	0	pos.	—	
18	SW	29.80	57	0	pos.	—	
19	SW	29.75	59	0	pos.	—	
20	S	29.57	59	0	pos.	—	
21	SW	29.48	56	0	pos.	—	
22	SW	29.25	55	small	—	neg.	A wet morning. Twelve o'clock, the rain fell more plentifully, and the rod was rather strongly charged negatively.
23	SW	29.24	54	strong	—	neg.	Half an hour after twelve o'clock P.M. an

Days.	Wind.	Barom.	Ther.	Sparks.	Pos.	Neg.	
		Inches.	°				extensive black cloud, without any breaks in it, approached the rod, by which it became strongly electrified, negatively; and in the space of twelve minutes a most awful darkness filled the atmosphere, and some rain fell, mixed with hail. The electric charge in the rod now became positive; and for the space of ten minutes it was as strongly electrified as it possibly could be; for I now counted 172 spontaneous explosions between the brass ball and bell; the bell ringing briskly all the time. These lucid sparks succeeded each other almost too quick to be noted. When the explosive sparks ceased, the electricity of the rod changed, and ended as it had begun, negative. A little after five o'clock fell a heavy shower of rain, and a second strong charge in the rod succeeded, attended with three gradual changes of kind.
April 24	SW	29.31	54	strong	—	neg.	A little after two o'clock P.M. fell a shower of rain, by which the rod became moderately electrified, negatively. The bell rang weakly. This charge in the rod continued four hours, without change of kind.
25	W	29.84	51	strong	pos.	—	A.M. Three o'clock P.M. fell a shower of rain, which was rather strongly electrified, negatively, attended with three changes of kind.
26	E	30.	53	o	pos.	—	Serene foggy weather.
27	NE	29.84	53	o	pos.	—	
28	N	29.81	52	o	pos.	—	
29	N	29.86	51	strong	—	neg.	A.M. from a shower of rain.
30	N	29.90	48	small	—	neg.	During the greatest part of the day. The weather showery.
May 1	N	29.80	48	small	pos.	—	A.M. Afternoon negative. The weather still showery.
2	N	29.85	48	small	—	neg.	A.M. A mild showery morning.
3	N	30.	45	o	pos.	—	A.M. but very weak. Fair weather to-day.
4	N	30.	45	o	pos.	—	The atmospherical electricity has been so very weak this day, that I was obliged to connect, with the hand-rod, a *lighted torch*, to ascertain the kind of electricity.
5	N	29.96	47	o	pos.	—	Fair serene weather these four days; and the atmospheric electricity constant.
6	N	30.	46	o	pos.	—	
7	N	30.28	45	o	pos.	—	
8	N	30.32	48	o	pos.	—	

A monthly account of sparks, and of positive and negative electricity, as indicated by the pith-ball electrometer, connected with the rod; excepting a few times, in very moist weather, in which it was obtained by the hand exploring-rod, with a lighted *torch* to it. See Fig. 2. Tab. IV.

		Times.		Times.		Days.
23 days of May, 1790, and 8 days of May, 1791,	Positive	40	Negative	27	Sparks drawn	13
June	Positive	45	Negative	22	Sparks drawn	5
July	Positive	36	Negative	23	Sparks drawn	8
August	Positive	33	Negative	6	Sparks drawn	3
September	Positive	39	Negative	11	Sparks drawn	19
October	Positive	37	Negative	7	Sparks drawn	22
November	Positive	30	Negative	8	Sparks drawn	11
December	Positive	35	Negative	11	Sparks drawn	6
January	Positive	28	Negative	8	Sparks drawn	3
February	Positive	36	Negative	12	Sparks drawn	6
March	Positive	34	Negative	8	Sparks drawn	2
April	Positive	30	Negative	14	Sparks drawn	8
		423 times		157 times		106 days

It appears, by comparing the monthly account of this year with that of the preceding, that there has been a considerable disproportion in the electrical positive state of the atmosphere; but which, when duly weighed, will not appear so very great as it now does. For when it is considered, that in the preceding year there were 73 days in which weak signs only of the electric fluid were observed; that seven days were destitute of electric signs; and that that kind of weather in which very weak signs of atmospherical electricity could be obtained, is now found, by a more sensible electrometer than was at that time used, to be always positively electrified, it will, I presume,

diminish the apparent disproportion. And as for the remaining difference, I also attribute a good deal of it to the accuracy of my present mode of obtaining atmospherical electricity, with a more complete apparatus; by which I have been able to collect the electric fluid, in sufficient quantity to ascertain the kind which predominates in the atmosphere, even in its weakest state. I have, therefore, found it an easy matter to fix the kind of electricity that the aqueous vapours in the air were charged with in each day throughout the year.

From repeated observations, and long experience, I am perfectly satisfied that the aqueous vapours, suspended in the air, are constantly electrified; requiring only the aid of a proper collector, to render the effects of their electricity at all times sensible. And for this reason, there may be justly said to be, an electrical atmosphere within our aerial atmosphere.

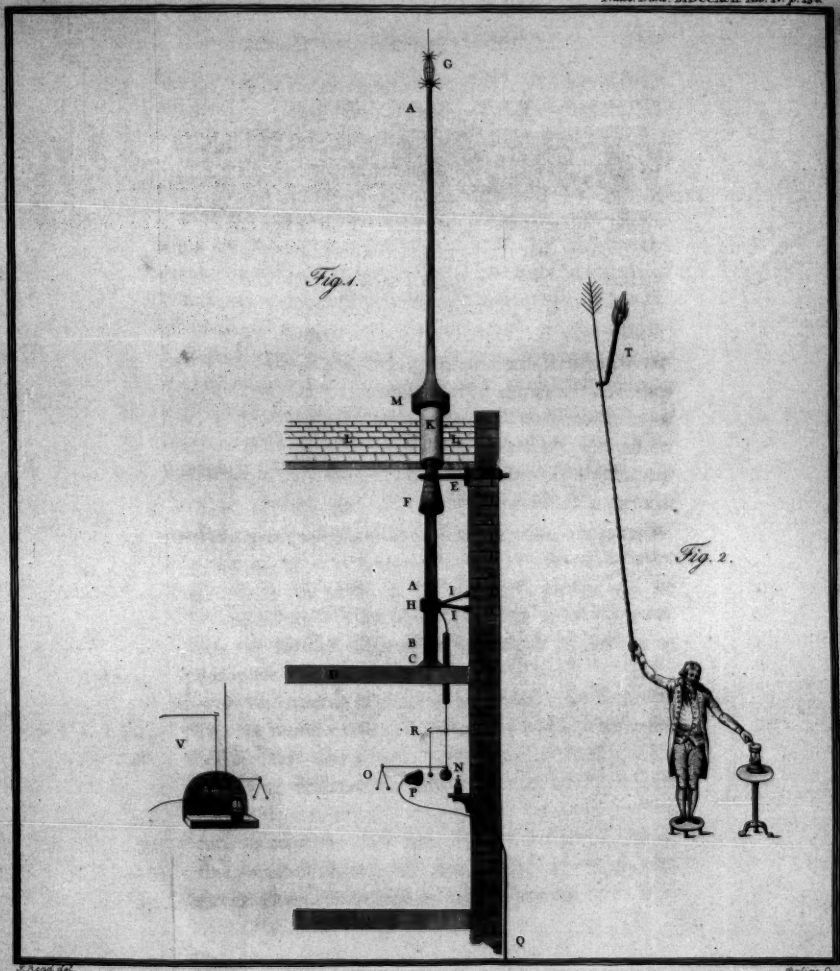
During a course of moderate weather, the electricity of the atmosphere is invariably positive; and exhibits a flux and reflux, which generally causes it to increase and decrease twice in every twenty-four hours. The moments of its greatest force are about two or three hours after the rising, and some time before and after the setting, of the sun; those when it is weakest, are from mid-day to about four o'clock. The periodical electricity of the atmosphere seems to be manifestly influenced by *heat* and *cold*. Hence it plainly appears, why we always find warm small rain to be but weakly electrified; when cold rain, which falls in large drops, is the most intensely electrified of any.

The abovementioned eighth day of May, completes my second year's journal; which, agreeable to my promise, I have now the honour of presenting to the Royal Society, with a

description of the improved atmospheric apparatus, which has been used for the foregoing observations.

I must now close that regular account which, for the last two years, has daily occupied my attention. Should any very remarkable appearances, however, in future take place, during a long course of frosty weather, which is the only state of the atmosphere that I have not yet experienced,* as my apparatus will continue in order, I shall carefully note them, and present them to the Royal Society: hoping that some more able philosopher may be induced to take his share of labour in this interesting and promising field, in order to investigate, as far as possible, the nature of so active, universal, and, in all probability, important an agent, as the atmospherical electricity seems to be; and to pursue the subject, with that diligence, and unprejudiced accuracy, which has been my principal object in the course of my observations.

* As I do not remember that there have been twenty-four hours of hard frost, during the two last winters.



F. Reed del. Basire Sc.

XII. *Further Observations on the Process for converting cast into malleable Iron. In a Letter from* Thomas Beddoes, *M. D. to Sir* Joseph Banks, *Bart. P. R. S.*

Read May 3, 1792.

DEAR SIR,

SINCE I described to you the process known among the workmen by the term, *puddling* of iron, I have several times reconsidered the explanation I attempted, in the same letter, of the phænomena it presents. My explanation could not indeed but be in great measure conjectural; and subsequent reflection excited in my mind a very lively wish to ascertain, in a decisive manner, the nature of the process. The following experiments will, I flatter myself, serve to determine the degree of confidence with which the principal points of my theory may be received, though they will not afford a solution of all the questions which my former communication might suggest to an acute philosopher.

They were undertaken in order to ascertain, 1. whether any elastic fluids are really extricated during the conversion of cast into malleable iron; and 2. what is their nature; and 3. whether they vary at different periods of the process, as I concluded from the appearances in the furnace. It seemed of less consequence to ascertain their quantity. I did not, however, neglect this object of inquiry, but you will find that some very curious circumstances prevented me from attaining it.

EXPERIMENT I.

Six pounds of dark grey melting cast iron were put into an earthen retort; a glass tube was luted to the neck, and its extremity was immersed in water. The retort was placed in a wind furnace. Before the retort and its contents could be supposed to be red hot, inflammable air came over. It burned with a deep blue flame, and was in no degree explosive. It rendered lime-water turbid, and was partly absorbed. When the retort had been heated about an hour and half, the air, which was coming over pretty copiously, that is, at the rate of an ounce measure every three minutes, upon an average, suddenly ceased, and the apparatus, on examination, proved to be no longer air-tight. The retort was found to be cracked; and the lumps of iron had none of them been melted, but they had been softened, and conglutinated together.

EXPERIMENT II.

Four ounces Troy of the same iron were put into one of Mr. WEDGWOOD's earthen tubes, glazed and closed at one end. That end of the tube was inclosed in a barrel-shaped crucible, the interstice filled with sand, and the crucible reclined so as to form a very small angle with the horizon: in other respects the apparatus was disposed as before. On the application of heat, air was again extricated, sooner than I should have expected, of the same inexplosive inflammable kind. About one-seventh of that which came over first, and which traversed the water of the receiving vessels, was absorbed by milk of lime. The residue burned slowly, with a flame apparently not so deep as before the carbonic acid was separated.

In this and the former experiment, the elastic fluids were

most rapidly extricated on the first impression of a red or white heat. Afterwards they came over much more slowly; during a considerable part of this experiment you might count twelve slowly between every air-bubble.

When the utmost power of the furnace had been exerted for three hours, a phænomenon occurred which produced some surprise in every person present; and there were several who had been abundantly accustomed both to chymical and metallurgical operations. A considerable absorption took place, and for about half an hour, it was necessary to blow air up the glass tube, to prevent the water from rising into contact with the iron. It afterwards appeared that the lead of the glazing was revived, which sufficiently explains the absorption.

586 grains only of the iron had been completely fused. The surface of two of the unmelted lumps was curiously covered with numerous small blisters of metallic lead.

About seven hours after the fire was first kindled, it was discovered that the apparatus had failed. I had examined the air that came over immediately before this accident, both by means of lime-water, and milk of lime, without discovering any vestige of carbonic air.

The iron weighed altogether three grains more than at first. But the adhering lead, and a quantity of lead also which was incorporated with the iron, concealed a real, and probably a considerable, loss of weight. The phænomena it exhibited, when put into weak vitriolic acid, and the vitriolated lead which was formed, indicated the presence of this metal in all the superficial parts of the mass. When it had been kept some time in vinegar, it dissolved readily enough in vitriolic acid at first, but the solution soon ceased, or became very slow.

EXPERIMENT III.

A coated flint glass retort was employed in this instance. The apparatus resisted a strong heat for two hours; and air, of the heavy inflammable kind, came constantly over.

EXPERIMENT IV.

A coated retort of crown glass, containing six ounces Troy of the same iron, was placed on a crucible nearly full of sand, and disposed as in the former experiments. I now wished to measure the quantity of the air, and I therefore determined to receive it in mercury. It would have been in vain to attempt this in water, on account of the carbonic acid air. About twelve o'clock the retort was judged to be of a dull red heat, and inflammable air came over. The orifice of the transmitting glass tube was now covered to the depth of half an inch with mercury, when the discharge of air instantly ceased: the lute seemed entire. Some of the mercury being removed, so as to leave just enough to cover the mouth of the tube, immediately the air issued again in bubbles, a proof that the apparatus was entire. The mercury was poured into the trough again, and in an instant there was a cessation of air. The mouth of the tube being uncovered, and a lighted paper applied, a blue flame appeared, and continued to burn, so great was the quantity of air discharged. The orifice of the tube was one-tenth of an inch in diameter. We found that this constant flame could be produced at any time during three hours and an half. When water was substituted in place of mercury, air issued slowly, and as if with difficulty, under a pressure of five inches. When only half an inch was left over the mouth of the tube, small bubbles ascended freely. During

a considerable time I counted four, slowly, between each of these bubbles. I did not collect above three ounce measures of air, and this contained carbonic acid. It was past four o'clock when the apparatus ceased to be air-tight, and the fire had been kept as strong as possible. The iron was most completely fused. There was a good deal of revived lead within the retort; there were also many globules in the neck. Probably some broken flint glass had been added to the usual materials for crown glass; I cannot otherwise account for the appearance of the lead here. In the last experiment the lead of the flint glass had been revived.

EXPERIMENT V.

Two ounces of the same iron, immediately upon being taken out of a retort, in which they had been kept, at a red heat, for about an hour and a half, and which were therefore as free from water as iron can easily be procured, were put into an earthen tube, unglazed, and closed at one end. This tube was disposed as in experiment II, only the end of the glass tube was immersed in mercury, instead of water. You will not be surprised to hear that air did not now come over so soon as in any former instance. When the fire was raised to its full force, exactly the same amusing variety of appearances took place as in the last experiment. Under the pressure of half an inch of mercury, not a particle of air was discharged; but the moment the pressure was diminished to a small fraction of an inch, the bubbles succeeded each other pretty quickly; and so on repeatedly. Upon lowering the surface of the mercury, and pouring some water upon it, I received more than two ounce measures of air, which, by the test of lime-water,

did seem to contain a vestige of carbonic acid, but it was too minute to be appreciated. This experiment with the air was made after a strong white heat had been kept up for three hours. Soon afterwards the bubbles ceased; but we could not then, nor upon examination of the apparatus when cold, discover any failure. The fire was still kept up for three hours. The tube must have been exposed to a strong white heat seven hours in all. The iron had lost eleven grains in weight. Only about one half had been thoroughly fused. The surface of two lumps, that had not been fused, had the close texture, and silvery appearance, of malleable iron. The thin edges yielded to the stroke of the hammer, and a gentleman, perfectly conversant in the nature of iron, agreed with me, that it had all the characters of malleable iron.

EXPERIMENT VI.

Thirty-one grains of artificial plumbago, in shining flakes from the iron works, were exposed in a small retort to a strong heat, for six hours, in the same pneumatic apparatus. It was difficult to separate, even by the help of the magnet, all the intimately mixed particles of iron, and there were also a few particles of coak incorporated with the plumbago. Air, of an explosive inflammable kind, was extricated, and rose freely through five inches of mercury. We had not been sufficiently careful to let the lute fix before we commenced the experiment, and it soon failed. Upon taking off the pressure of the mercury entirely, and repairing the lute as well as we could, we had every reason to believe that the air soon ceased. The air received in the mercury contained one-eighth of carbonic acid. The remainder exploded. The plumbago lost four

grains. Mr. PELLETIER, if I remember right, found that native plumbago, exposed to a fierce and long continued heat, lost 10 grains in 200. In the present experiment, its appearance was unaltered. Probably the loss was owing to moisture imbibed by the particles of coak, and to a small combustion by the air in the retort.

It will, I think, be admitted, that these experiments abundantly confirm the inferences I had formerly drawn from appearances by their nature less decisive. The real extrication of air, varying in its nature at various periods of the process, seems to be placed beyond doubt. The experiments in glazed and glass vessels, were made with a view to exclude the possibility of the supposition of the air entering through the pores. I think that Dr. PRIESTLEY, if he should repeat these experiments, and find that they have been accurately made, will, with his accustomed openness to conviction, abandon an opinion he has for some time entertained, and no longer consider water as essential to the constitution of elastic fluids. Several observations might be made upon this point, and those which I have just noticed above; but they will readily occur to persons conversant in chymistry, and it is not the object of the publications of the Royal Society to teach the elements of science. I shall, therefore, confine myself to the unexpected and anomalous appearances, and then attempt to draw a few useful inferences.

1. I was surprised at the extrication of inflammable air in such low degrees of heat. You have seen that cast iron, highly charged with charcoal (the *phlogisto onustum* of BERGMAN) yields air at the temperature of melting lead. For undoubtedly the blisters of lead, which lay upon the iron, are to

be considered as air bubbles caught in a solid film of lead. Perhaps white cast iron would not yield air so readily; possibly iron holds its charcoal with more force as it contains less. I intend to make some comparative experiments upon the varieties of cast iron, but so curious an appearance as these blisters, will always be rather the bounty of accident, than the effect of skill or labour. The obvious method to produce them, would be to cover the iron with lead. All the unmelted lumps of iron were porous, and open in their texture.

2. I am at some loss how to explain the occasional discharge and cessation of air, in one experiment in which a crown glass retort was used, and in another with an unglazed earthen tube. There was no flaw in the lute, nor in the vessels, for it was discharged for the space of several hours under a small pressure. Either, then, it was forced through the softened glass in the first, and the dilated pores of the tube in the second case; or it was absorbed by the substance of the vessels; or it was not extricated from the iron. Of these suppositions the third seems to me the most probable. It is not likely that an hole should be made through the melted glass, under the pressure of the half, and closed under that of perhaps the eighth of an inch; or that pores in the tube should open and shut in conformity to such a variation of circumstances: and, with regard to the tube, there can be no question as to absorption. One principal difficulty, as it appears to me, in the manufacture of iron, is to get rid of the charcoal. The oxygène readily enough unites with a small portion; but the attraction of the iron on the one hand, and on the other, the little disposition of the charcoal to put on the elastic form, in comparison with many other less fixed substances, together form a

very considerable obstacle to the change of charcoal into air; and as I have already observed, the iron probably holds the charcoal more strongly as its quantity diminishes. In this state of things a small additional impediment will prevent the heat from throwing the charcoal into the state of air; and some degree of pressure must be adequate to this effect: and why may not this point, from which as you recede on opposite sides, the attraction of the particles of charcoal for one another, or for iron, either shall or shall not be overcome by heat, have been found in these experiments? The next consideration will both illustrate and confirm these ideas.

3. A chymist, whose notions of iron are derived principally from books, and from the phænomena which are presented by processes not having metallurgy for their immediate object, will be apt to consider some things related above as inconsistent: the violence of the heat, for instance, and the smallness of its effects; since even cast iron was not fused in all the experiments. The fact is, when cast iron exposes a large surface, and heat is gradually applied, it proves almost as infusible as malleable iron: indeed, by the gradual action of heat it is converted, superficially at least, into malleable iron, or approximates towards it: and considering only iron and charcoal, I believe, *the fusibility of iron will be directly as the quantity of charcoal it contains.* Now in the experiments I have described, pieces of one, two, and three drachms, and sometimes less, were used, for larger could not be inserted into the neck of the retort. And, in order to avoid this inconvenience in future, I would recommend cylinders to be cast, of a diameter suited to the mouths of the vessels. This infusible coat would be an impediment to the conversion of the parts below, by

pressing upon them; the elastic fluids could not either traverse the solid surface so freely as a liquid, and perhaps, as I am disposed to believe, they could not traverse it at all. The malleable skin seems close in its texture, and the porosity of the rest might arise from the generation of just air enough to produce an internal expansion. In the puddling operation, it is of the most material consequence to keep the mass in constant agitation. Thus the parts are thoroughly blended, the attraction of cohesion is a good deal counteracted, and there can be no pieces hide-bound, if I may so express myself. This last, perhaps, is the greatest advantage derived from the labour of the workman.

4. I was asked by one of the most ingenious and profound philosophers of the present age, why I had neglected the action of the atmospheric air in the theory of the conversion of iron? It is simply because its action upon the metal seems, in practice, pernicious; I consider its presence as an evil, though a necessary one, according to the present modes of working; I was also anxious to try this opinion by the test of experiment, and you see it has been fully confirmed. In the last experiment, part of the iron was completely converted, and in some others it seemed approaching fast towards *nature*, as the manufacturers express it. It is, indeed, very possible to conceive a way in which air might be beneficial; that is, if it could be applied so as to burn the charcoal merely; but at present, for one grain of charcoal which it converts into carbonic acid air, it converts many of iron into finery cinder; and as I have formerly shewn, this is not the way in which iron is actually converted in the reverberatory, and probably not in the finery, furnace.

5. It is impossible to ascertain the principles of any art, without immediately improving the practice, or opening a prospect of future improvement. The preceding observations may serve to direct attempts to render the metallurgy of iron less difficult, laborious, and expensive. For, 1. if a quantity of oxygène, nearly sufficient to burn the charcoal, could be chymically combined with the cast iron, the operation would consume less fuel, and would not require so long a time. It may be worth while to consider if the ores of iron, containing manganese, owe any part of their value to this circumstance. 2. If it could be contrived to apply a sufficient heat to large quantities of iron in close vessels, and at the same time, to agitate them sufficiently, the loss in conversion would not, perhaps, exceed ten in an hundred. 3. The important object of converting British iron into steel, may possibly be attained by following up reflections suggested by the foregoing experiments. When the oxygène has been separated in the form of carbonic acid, there will remain the charcoal and iron, the constituent parts of steel. Perhaps the materials, at a certain period of the process, may be so nearly approaching to steel as to be easily convertible. The mass will contain also a quantity of sulphur, on which perhaps the difficulty of making good steel from our iron depends. But this difficulty, I am persuaded, will not be insuperable.

It may be proper to add, that whenever attention was paid to it, the hepatic smell in the extricated air was perfectly distinguishable.

I hope you will also permit me to add, that whatever information or advantage may be derived from these facts and

observations, must be in a great measure ascribed to the liberal curiosity of WILLIAM REYNOLDS, whose enterprising spirit and inventive genius have improved our machinery, enlarged our manufactures, and changed the face of a large district in his native county.

I have the honour to be, &c.

Shifnal, Shropshire,
February 14th, 1792.

THOMAS BEDDOES.

P. S. The residuum of 486 grains of cast iron, the same as that used in experiment 1. weighed 48½ grains, after being dissolved in weak vitriolic acid, and heated to a dull red heat; the same quantity of iron, after the experiment, afforded a residuum of 39 grains, and a little more; in the residuum left by equal quantities of iron, before and after the experiment in the unglazed tube, there was a difference of five grains; the solution of the iron that had been submitted to the experiment went on very slowly; and would not have been effected by vitriolic acid in many months. In the latter case, I used some muriatic acid, which quickly dissolved it: in the former, weak aqua regia was used for the solution of a very small part of the whole lump. I suspected lead to have caused the slowness of the solution in the first case, but there can be no such suspicion in the second. The difference between these residuums tends to shew that plumbago was consumed by the heat: but they do not shew the loss accurately; for in the residuum of the iron that had been fused in the first experiment, there was a small quantity of vitriolated lead; and in the other

there was, besides the plumbago, a small quantity of that difficultly soluble calx of iron, which the solutions of this metal deposit on long exposure to air. The difference was greater, therefore, than it appeared. On the other hand, the long action of the acids might have consumed some plumbago. There was little or no calx attractable by the magnet in the residuums of the fused iron. From the 48 grains of residuum, I separated more than six grains by the magnet.

XIII. *Continuation of a Paper on the Production of Light and Heat from different Bodies.* By Mr. Thomas Wedgwood; *communicated by Sir* Joseph Banks, *Bart. P.R.S.*

Read May 10, 1792.

EXEPRIMENT I.

In order to discover what effect the *light* of the burning fuel has upon incombustible bodies, I fixed into the end of a tube of earthen ware * two equal cylinders of silver, with polished surfaces, half an inch in length, and a quarter of an inch in diameter (see Tab. V. Fig. 1.); one of the cylinders was painted over, except the end within the tube, with a thin coat of incombustible black colour, to make it *absorb* the incident light; the other, intended to *reflect*, was left with its polished surface. Applying my eye to the opposite extremity of the tube (which it fitted exactly, so that no extraneous light could enter), and directing it towards the two polished ends of the cylinders, I held the tube within a red hot crucible, surrounded by burning coaks, and continually turned it round, that both cylinders might be equally exposed to the light, and heat. The result was, that the end of the blackened cylinder began to shine a considerable time before that of the polished one, and remained constantly somewhat brighter: on removing the tube

* When *earthen ware* is mentioned in this paper, the cream-coloured or queen's ware is meant.

from the crucible, still looking within it, I was surprised to see the appearance reversed, the polished cylinder continuing to shine for some time after the blackened one had ceased. Cylinders of gold, and of iron, treated in the same manner, gave the same general result; but the differences between the polished and the blackened ones were not so remarkable in these, as in the silver.

I repeated this experiment many times, and found, by observations with a stop-watch, that the blackened silver cylinder began to shine, at a medium, in two-thirds of the time which the polished one required; and that, after its removal from the crucible, it continued to shine only two-thirds of the time that the other did. For this latter observation, I was obliged to make a little variation in the apparatus; the tube itself becoming frequently so hot, as to make the cylinders continue longer red than they otherwise would have done: I therefore took them out of the tube, to suspend them by a fine wire, and then heating them in the ignited crucible as equally as possible (for they cannot be made to exhibit to the eye the same precise tinge of redness), I removed them immediately into a dark place.

From this experiment it would seem, that a great part of the *light* emitted by the cylinders was absorbed from the red hot crucible, as the blackened one, which absorbs most rays, not only became first red, but likewise shone brightest. The following experiment, however, affords a different conclusion.

EXPERIMENT II.

An earthen ware pipe, of a zig-zag form (Fig. 2.), was placed in a crucible, which was filled up with sand, the two open

ends of the pipe being left uncovered; one of them was of a proper form for receiving the nozzle of a pair of bellows, the other bent into angles of the form of the letter Z: on this last was fastened a globular vessel A, with a lateral bent pipe, to let out air but exclude all external light, and with a neck in which was inserted a circular plate of glass. The crucible, with the sand and the part of the pipe contained in it, was then heated to redness. Having my eye fixed in the neck of the vessel A, and observing it perfectly dark within, I directed an assistant to blow with the bellows. The stream of air, sent through the red hot tube, not being at all luminous, I fixed a small strip of gold into the orifice of the tube at B, which, after two or three blasts, became faintly red; thus proving, that the air, though not luminous, was equal in temperature to what is usually called red heat. I then heated the crucible to a brighter redness: the stream of air, blown through the bright red hot tube, still came out perfectly dark, but the strip of gold, exposed to it, shone both *sooner* and *brighter* than before.

Hence it appears, that the greater brightness of the blackened cylinder, in the first experiment, was owing to its being of a higher temperature; and that it would have been equally bright had it been raised to the same temperature by any other means than the absorption of *light*; the metal being here brought to a faint, and to a bright ignition, without the access of any visible light.

But perhaps another consequence may be fairly drawn from this experiment. As the gold may be made to emit *light* for any length of time, by being supplied with *heat* from the *dark* air of the temperature of red heat, neither the gold nor

the air suffering any chemical change whatever, is not the *light emitted* identical with the *heat received*? This identity appears to be confirmed by the following observation: that if the solar rays be made to converge upon one end of a blackened cylinder of metal, the other parts will become red hot, and emit light; or, if the rays be converged upon the tube blackened, and air passed through it, the gold placed in the dark current will yield a constant light.

The simultaneous *absorption* and *emission* of light, in a red hot body, is a subject of very difficult and abstruse investigation, as it involves the nature of the constituent parts of matter, and of their relative actions and arrangements. I shall not attempt to offer any hypothesis for explaining the various phænomena, as I have not been able to form one at all satisfactory to myself; but shall proceed to state a few miscellaneous experiments and observations, which, though apparently unconnected, may yet be of some assistance to the speculation of some abler theorist.

EXPERIMENT III.

A quart of oil was poured into a bright tin vessel, which had a FAHRENHEIT's thermometer fixed in its neck. The mercury standing at 45°, the vessel was plunged into boiling water, and the time which elapsed before the mercury rose to 180° was exactly noted. I then blackened the exterior surface of the tin vessel, and, repeating the experiment, found the thermometer to require exactly the same time as before, to rise to the same degree.

From the foregoing experiment it appears, that *black* matter

has no particular attraction to light in a *quiescent* state, that is, when combined, as heat, with other matter.

EXPERIMENT IV.

Three equal cylinders of glazed earthen ware were fixed in the end of a tube (like the two silver ones in Fig. 1.); one of them blackened; another gilt, all but the ends within the tube; and the third with its glassy surface. These, treated in the same manner as the silver cylinders, in the first experiment, all became *red* at the *same time*. Without taking them out of the tube, I removed the whole from the fire, and, still keeping my eye upon their ends, observed them all to disappear together.

To account for the simultaneous ignition of these three cylinders, it must be considered, that earthen ware being a very slow conductor of heat, the surfaces of all of them are, probably, heated to redness some time before any such appearance takes place in the ends within the tube. Now it is not unlikely, that the black matter, the gold, and the glaze, when red hot, may reflect light equally; and, in that case, there should be no perceptible difference in the time of ignition of the ends within the tube, except the little advantage gained by the blackened one before its black coat becomes red hot, which is partly counterbalanced by the powdery matter (of which the coat consists) obstructing the transmission of heat. The surfaces of the silver cylinders (experiment 1.) on the contrary, do not become red any sensible time before the ends within the tube, the metal conducting the heat rapidly through its whole mass.

An earthen ware cylinder, fastened in the end of an earthen

ware tube, along with a gold one, and plunged into melted glass, is much longer in heating than the gold one. This is easily explained, upon a well known principle, namely, that in two bodies of unequal temperature, the colder body conducts the heat from the hotter at a rate directly proportionate to their difference of temperature. Now the surface of the earthen ware cylinder, as the heat is conducted very slowly from it by the interior mass, soon becomes very nearly of the temperature of the hot glass contiguous to it. The surface of the gold cylinder, on the contrary, having its heat conducted from it much faster by the interior mass, is of consequence disposed to receive the heat with greater rapidity.

EXPERIMENT V.

Equal pieces of gold, silver, copper, and iron, blackened all over, and suspended by a wire in a red hot crucible, became red in the order in which they are here set down; and when made equally red, and removed into the dark, they disappeared in the same order. When just brought out of the fire, they all looked equally red; but when they had cooled a little, the iron was much the brightest.

An earthen ware cylinder, tried with the metals, disappeared much sooner than any of them, the interior part not communicating its heat quick enough to keep the surface of the temperature of red heat: accordingly, when broken, though the surface gave no light, the mass was luminous internally.

From a parity of reasoning, a gilt earthen ware cylinder, suspended in a red hot crucible along with a gold one, would probably become red on the surface before the gold one.

EXPERIMENT VI.

A tube of unglazed earthen ware, open at top, and having one half of its bottom blackened on the outside, was placed in a red hot crucible, and the eye directed, as before, to the inside: the part which was externally blackened became always red before the other.

The experiment was repeated with a metalline tube; but no difference could here be perceived between the blackened and unblackened half of the bottom. The reason is obvious, from the foregoing observations.

EXPERIMENT VII.

To ascertain whether metals and earthy bodies *begin* to shine at the same temperature, I gilded, in lines running across, a thin piece of earthen ware, of the specific gravity of about 2,000, and luted it to the end of a tube, the gilt side being inwards; then, directing my eye into the tube, I held it within a crucible, which was gradually made red hot; but I could not, after many trials, perceive that either the gold or the earthen ware began to shine first.

As it appears, from this experiment, that gold and earthen ware begin to shine at the same temperature; and as no two bodies can well be more different, in all their sensible properties, may it not be inferred, that almost all bodies begin to shine at the same temperature?

EXPERIMENT VIII.

Observing that colourless transparent glass had a paler hue, when red hot, than most other bodies, I conceived that it might

not be luminous at so low a temperature. I therefore took a circular piece of glass, about $\frac{1}{40}$ of an inch thick, and having gilt one side of it, exposed the ungilt side to a stream of air passed through a red hot tube; but did not perceive that the gold shone at all before the glass. This experiment, however, is not decisive; glass being so slow a conductor of heat, that its exterior surface might have been heated some time before the interior, and thus have deceived the eye. I could not meet with any glass sufficiently thin for this purpose, nor think of any other possible mode of trial.

EXPERIMENT IX.

Having often remarked, that the surfaces of red hot metals had an appearance different from what they present by reflected light when cold, I had an idea that this peculiar appearance might be derived from a transmission of the light through the superficial parts of the ignited body. To ascertain whether they acquired any degree of transparency by heat, I fixed a circular plate of fine gold, about $\frac{1}{60}$ of an inch thick, on the end of a tube, which was perfectly closed by it; then, having heated it to redness, and looking down into the tube, I pressed the outer surface of the gold against single grains of gunpowder: the red light of the gold looked whiter on every flash. To be satisfied that no light found admission through the sides of the tube (which were of thick earthen ware), I covered the exterior surface of the gold plate with a thick coat of clay luting, and again making it red hot, fired gunpowder with it as before, but no increase of light was now perceptible from the flash; which proves, that the sides of the tube were impervious to the light. When this gold

was cold, I stuck a few grains of gunpowder upon its surface, and looking within the tube, fired them by pressing them against a hot iron, but the light of the explosions was not then sensible.

Plates of silver, and of iron, gave the same results.

EXPERIMENT X.

A lump of the most luminous marble, and an equal lump of the same marble blackened over, were placed together upon a mass of iron heated just under redness: the former gave out much light, the latter none. Upon a second exposure, the lump not blackened gave a faint light; the blackened one, as before, none at all. Then wiping off the black, and placing them together upon the heater, I found the one which had been blackened to emit as little light as the other: thus the phosphorescent property was nearly destroyed, without any visible light leaving the body.

EXPERIMENT XI.

If a piece of glass, or glazed or unglazed earthen ware, with any enamel, painting, gilding, or writing in ink upon it, be made red hot, the coloured parts appear considerably more red than the others, and continue longer visible. Iron wire, within a red hot glass tube, looks much more red than the glass. Black matter, upon a large polished mass of fine gold, did not remain any longer red than the gold.

EXPERIMENT XII.

A bit of iron wire becomes visibly red hot when immersed in melted glass. Air, therefore, is not necessary to the shining of ignited bodies.

EXPERIMENT XIII.

A piece of red hot metal continues to shine for some time after its removal from the fire; which proves, that constant accessions of light or heat are not necessary to the shining of ignited bodies. If the piece be strongly blown upon, it instantly ceases to shine; for the cold air, continually applied, unites with the light as fast as it leaves the body, and which otherwise would have passed to the eye.

I shall now close this paper with two or three miscellaneous observations.

Red hot bodies, though ignited by *white* light, give out only the *red* rays. Perhaps the other more *refrangible* rays, from their greater attraction to matter, may be circulating as heat, whilst the red ones, having a less attraction, yield sooner to that force which propels the light of red hot bodies. If the intensity of the incident white light be much increased, so as to raise the body to a white heat, the more refrangible rays then come out with the others, constituting together a white light.

The flash of a grain of gunpowder is a pure *white* light: but if the explosion be made within a thin, unglazed, earthen ware tube, close at both ends, all the light that pervades the sides of the tube is red: the other rays must, therefore, remain united with the matter of the tube, whilst the less attractive red ones are transmitted. Thus also, on looking at the sun through the thin bottom of an earthen ware tea-cup, only the red rays are transmitted, so that the others must be retained by the matter of the cup.

It would perhaps be worth trying, whether a body can be made *red* hot by concentrated rays of other colours.

The light produced from bodies by attrition consists of a double light; that which their powder would give out on the heater under redness; and that which particles in their surfaces give out by being made red hot. The sudden heating of a body to redness, by a single rub or blow, is a remarkable phænomenon, and deserves to be investigated. One effect produced upon a body by attrition, is a compression or condensation of the parts in its surface; and it appears from general observation, that a condensation of the parts occasions a diminution of its capacity for heat. Iron may be made red hot by repeated blows of a hammer; and I have found, that if red hot iron be forcibly struck by a heavy hammer, with a sharp edge to concentrate the action, the part so struck emits a white light for a sensible time, and is probably raised to a white heat: also, that my father's thermometer clay has its capacity for heat diminished one-third, by being burnt to 120° of his scale, and thus reduced to about one-half of its bulk; and as it loses in weight little more than two grains on a pound, the diminution of capacity can only be attributed to its condensation. Many other analagous instances might be adduced if necessary;* but these will, perhaps, be deemed sufficient to render it probable, that the sudden ignition of the particles by attrition proceeds from the compression, and consequent diminution of the capacity for heat.

I am not certain that the increase of brightness in the gold plate, experiment IX. must be attributed to its transparency: it may arise from the gold being suddenly heated to a white heat by the light of the explosion; or the force of the explosion

* See Dr. DARWIN's excellent paper of Frigorific Experiments on the mechanical Expansion of Air, &c. Phil. Trans. Vol. LXXVIII. p. 43.

may condense its parts, and diminish its capacity for heat or light. There is, however, a strong analogical argument for the transparency of the gold: every body whatever, when extremely thin, is pervious to light in such quantity as to be perceptible to our eye-sight: thus gold, perhaps the most opake of all bodies, platina excepted, when beaten into leaf gold, is so pervious to the green rays, that, if held close to the eye, all objects are seen through it with considerable distinctness, appearing of a deepish green hue. Now the particles of matter in the gold plate being much separated from one another, if not more regularly arranged, by the heat; and the intensity of the light in the explosion of the grains of gunpowder being so great; it is not improbable that some few rays may be transmitted through the gold.

After some reflection upon the curious result of experiment 1, I am inclined to think, that the blackened cylinder does not begin to shine at so low a temperature as the polished one; and, consequently, that the *commencement* of ignition is not, in all cases, a certain indication of a particular temperature. For, when the two cylinders were removed from the ignited crucible (see Fig. 1.) the blackened one looked of a brighter red than the polished, and yet, in the course of cooling, disappeared in about two-thirds of the time that the polished one continued to shine, without any apparent reason for its cooling at a faster rate. Should it not, therefore, seem that it requires a greater heat to make it shine?

I am well aware, that these appearances may be differently explained; and, to determine this point, I would propose the following experiment. Put larger cylinders into the tube; and, having made them red hot, drop them separately, each at the

instant of its disappearing, into cups of weighed water, of the temperature of between 211 and 212° of FAHRENHEIT: then, as any addition of heat will expand the water into steam, the loss of weight of each vessel will give an exact measure of the heat of the cylinders at the time of immersion.

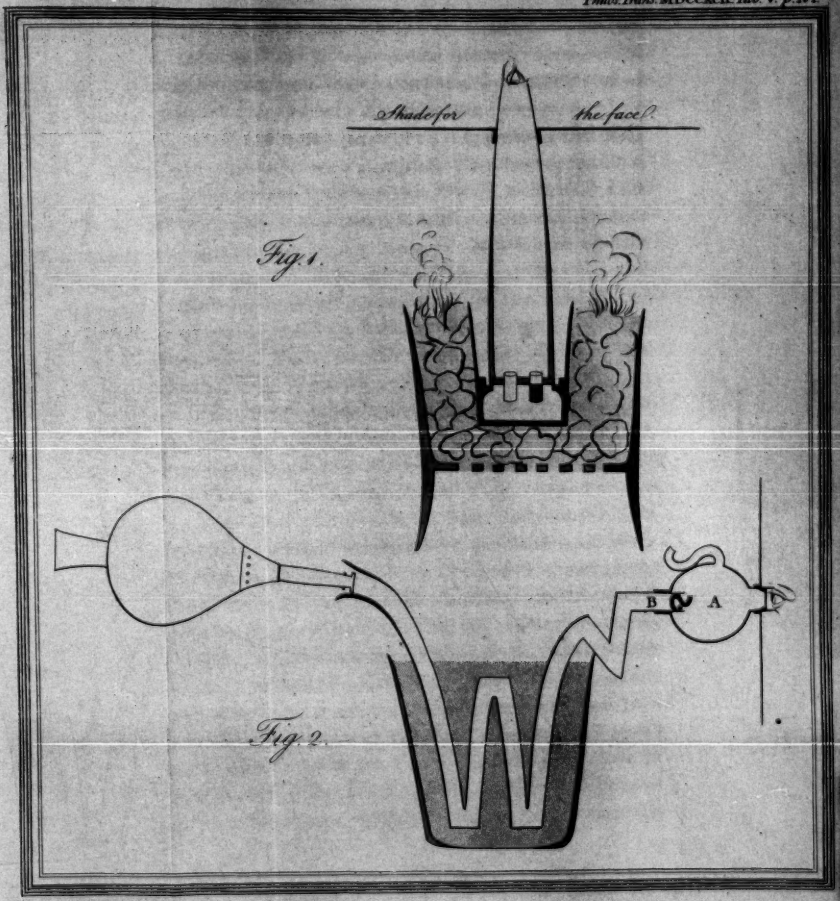


Fig. 1.

Fig. 2.

XIV. *A Narrative of the Earthquake felt in* Lincolnshire, *and the neighbouring Counties, on the 25th of* February, 1792. *In a Letter from* Edmund Turnor, *Esq. F.R.S. to Sir* Joseph Banks, *Bart. P.R.S.*

Read May 10, 1792.

DEAR SIR,

WHEN I heard of the earthquake of the 25th of February last, which was felt in Lincolnshire, and some of the neighbouring counties, about a quarter before nine in the evening, I immediately wrote to my friend Captain PENNYMAN, at Little Paunton, near Grantham, desiring him to inform me of the duration and extent of the shock, the direction it came in, and the state of the weather before and after it was perceived. His remarks, together with the subsequent information I have obtained, great part of which I derive from your obliging communication, may serve to record this recent instance of a phænomenon to the terrors of which this country is but little exposed.

Lord RAWDON's steward was sitting with his back against an old massy stack of chimneys, which gave him a sensible shove; he at the same time saw the chairs of other persons in the room move very visibly. Donnington Castle, Leicestershire.

Lord Rawdon's information to Sir Joseph Banks.

Lyndon, Rutland. An earthquake was felt all over this county. What I felt, and it was the general description given of it, was, that it came with a rattling noise, something like wheels running over a pavement, and a trembling shake, which those who were leaning against a wall felt more than those who were standing, or sitting upright. It lasted a considerable time; different people estimated it from a quarter of a minute to a minute. Several thought the noise and shake came from the north or the north-east; and indeed, I think I heard more of it from the northward than the southward of us. Some thought they heard a thump, as of a heavy thing falling down; but the rattling and shaking were much more taken notice of. Several persons at first suspected it might be a ball of fire, but no such thing was seen; and many were immediately sensible it must be an earthquake, especially those who had felt one before. No material hurt was done by it; some who were standing were seen to reel, and one who was walking was thrown against a wall by it, but not hurt; a stack of wood was thrown down, and some said a chimney. I heard of it as far as Newark, in Nottinghamshire; and it was said to be felt at Biggleswade, in Bedfordshire. The season was at the conclusion of a frost, there had been a little rain, and a thaw was beginning. The barometer gradually fell from the 23d to the 26th—the thermometer was as under:

Newark.

Biggleswade.

21st, morn. 16.5; 22d, 27.3; 23d, 22.3; 24th, 24.0; 25th, 34.9; 26th, 39.8; 27th, 40.2
aftern. 28.0; 32.6; 33.2; 39.0; 38.0; 45.5; 52.2

I felt the earthquake of September 30, 1750, as I did this, and heard the same rattling noise, and my seat shook under me. Many others heard and felt the same; but some

described it as the falling down of a weight. September, 1750, was almost all a calm, dry, and hot season, scarce a breath of wind, or a glimpse of sun. The end was cooler; but dry and calm, as before. The 30th was cloudy and calm, but there was a gentle wind for some time after the earthquake.

Thomas Barker, Esq. in a letter to Mr. Turnor.

Nottingham. The earthquake at Nottingham, as felt by Dr. WHITE, was undulatory in both shocks; but, in the second, he perceived the direction of the shock to be a little inclined to the horizon, towards the south-east. Langar. At Langar, its noise was like that of a heavy carriage driving rapidly along. Here the shock was felt by one or more persons, but not by all in the same room. Derby. Its direction was from west to east. It was felt at Derby very slightly; and was of no great extent from north to south in these parts, as it was not felt at Harborough to the south, nor at Chesterfield to the north. Stamford. It was sharply felt at Stamford, Wandesford. and Wandesford, but not at Alconbury.

The Rev. Edw. Gregory's information to Mr. Turnor.

Belvoir Castle. The shock seemed to be perpendicular, up and down, as if part of the castle had given way; no noise was perceived but wind.

Mr. King's information to Mr. Turnor.

Little Paunton, Lincolnshire. The morning presented a thick fog, which continued all the day; the air extremely heavy and close. About half past eight in the evening a noise came on resembling thunder, which lasted nearly half a minute, when two undulatory motions, in the quickest succession, were felt; a gust of wind, for some seconds, instantaneously succeeded, and in about five minutes

it rained heavy and large drops, which continued some time. The windows, window-bells, pewter upon shelves, &c. gave ample evidence of the agitation. Its direction was from north-
Branston. west to south-east. It was slightly felt at Branston, though not at Lincoln.

Captain Pennyman, in a letter to Mr. Turnor.

Belton. We were all much alarmed by a violent and sudden shock of an earthquake; it made the house at Belton shake, and was followed by a violent gust of wind, and a noise like thunder, which lasted about six or seven seconds. I really thought my chair was sinking into the ground, and, after I had recovered myself, I was not convinced that one of the fronts of the house, or a chimney, had not given way, till I had been in all the rooms, where I found every thing in its place. All the other servants felt it as I did, and the neighbours in the village thought their houses were going to fall upon them. It was
Grantham. Manthorpe. felt at Grantham, Manthorpe, and other towns in the neighbourhood.

Chr. Driffield, in a letter to Lord Brownlow.

Peterborough. An uncommon rumbling noise was heard round my premises, 110 yards, resembling that occasioned by drawing a large garden roller slowly over the pavement. The sound did not appear like thunder over the head, but upon or near the ground. The like, in every respect, happened October 27th, 1776, about a quarter before ten at night.

Rev. Archdeacon Brown, in a letter to Mr. Turnor.

Boston. The earthquake was felt and heard by a lady at Boston, who declared so at the time it happened, and said, she was

sure it was something extraordinary, though no one else in the town observed it, or paid any attention to her, till the news of its having been felt at other places arrived.

Mr. Fydel's information to Sir Joseph Banks.

The Transactions of the Royal Society give an account of the earthquakes in the northern parts of England, in the years 1703 and 1750. That of the latter year is described as "begin-" ning in Derbyshire, and passing off the island, through Lin-" colnshire and part of Cambridgeshire, its direction being from " west to east." * From the preceding narrative it appears, that nearly the same tract of country was affected by the late concussion, and that it came in the same direction from west to east; circumstances which correspond with the observations of Mr. MECHEL; 1st. " That the same places are subject to re-" turns of earthquakes at different intervals of time;"—2dly, " That earthquakes generally come to the same place from " one and the same point of the compass." These, and other facts, that ingenious philosopher adduces in support of his hypothesis, that earthquakes are caused by the steam raised by waters, contained in the cavities of the earth, suddenly rushing in upon subterraneous fires; which steam, the moment it is generated, insinuates itself between the strata of the earth, and produces the undulatory motion beforementioned. It may, however, be remarked that the state of the air, before the shock, was calm, close, and gloomy, such as is described by Dr. STUKELEY as necessary to prepare the earth to receive an electrical stroke, and the circumstance of its having

* Phil. Trans. Vol. XL. p. 722.

been partially felt in the same room may be supposed to favour that hypothesis; but yet the concussion seems not to have been so strong on the eminence at Belvoir Castle, as it was in the neighbouring vale.

I have the honour to be, &c.

March 10, 1792.

EDM. TURNOR.

XV. *Experiments made with the View of decompounding Fixed Air, or Carbonic Acid.* By George Pearson, *M. D. F. R. S.*

Read May 24, 1792.

From a Paper read to the Philosophical Society of Edinburgh, in 1755, published in the second volume of the *Physical* and *Literary Essays*, Doctor Black appears to have discovered the affinities between an aëriform substance, which he called *fixed air*, and alkalies, quick-lime, and magnesia. His experiments also shewed, that many properties of these bodies depended upon the union and separation of this air. The discovery of these facts established this elastic fluid to be a peculiar species of substance.

Mr. Cavendish, Dr. Brownrigg, Dr. Priestley, Sir Torbern Bergman, Mr. Bewley, Mr. Kirwan, and other chemists, afterwards extended, very considerably, the history of fixed air. The question, whether it was a simple or compound body, was discussed; and by many persons it was believed to have been proved, that fixed air was composed of phlogiston and respirable air. But some of the principal facts, upon which this belief was founded, being afterwards demonstrated to be erroneous; and the production of fixed air being, to the apprehension of many chemists, more satisfactorily accounted for by the new principles of chemistry, this doctrine of its composition became no longer tenable. As the science of chemistry ad-

vanced, many acids were demonstrably proved to consist of a peculiar basis, and respirable air; and on the ground of analogy it was concluded, that all other acids were composed in a similar manner. Fixed air having been shewn, by Mr. BEWLEY, and by BERGMAN, to be an acid, of course its composition was considered, in the new system of chemistry, to be similar to that of all other acids. On examining facts already well ascertained, and by various experiments discovering others, no clear instance could be perceived of the *formation* of fixed air, but in those cases where charcoal was applied red hot to respirable air. Mr. LAVOISIER at last established this interesting fact, by a conclusive experiment, published in a volume of the Memoirs of the Academy of Sciences in 1781, and in his *Traité Elémentaire* in 1789, by which he demonstrated that charcoal of wood, except a minute portion of residue, as might reasonably be expected, combined with respirable air, and composed fixed air only. This is the date, therefore, of the discovery by synthesis, of the composition of fixed air; or, as I would rather call it, with Mr. LAVOISIER, carbonic acid. The proof by analysis, however, was required, to render the demonstration of the composition of this elastic fluid complete. The honour of the first analytical experiments on carbonic acid is due to Mr. TENNANT, F.R.S. who, in a paper read to this Society, in March, 1791, and contained in volume LXXXI. of the Philosophical Transactions, asserted, that charcoal and phosphoric acid were produced by applying phosphorus to red hot marble; from which he inferred, that the carbonic acid of the marble was decompounded. This decomposition, the ingenious author conceives to be effected by the united powers of affinity between phosphorus and the respirable air of the carbonic acid

in the calcareous earth, and between the phosphoric acid, thus composed, and the quick-lime of the calcareous earth. That the black matter produced is really charcoal, the author has proved by adequate experiments. The inference, however, does not appear to me to be just, that the charcoal and phosphoric acid are the necessary result of the agency of the affinities, as stated by Mr. TENNANT. For the well known fact, that phosphorus cannot be produced from bone-ashes by the application of charcoal and heat, I think, only proves that the powers of affinity between respirable air and phosphorus, together with the affinity between the compound formed by their union (namely, phosphoric acid,) and quick-lime, are *not inferior* to the joint affinities between the respirable air, in the phosphoric acid, and charcoal, and between the compound of respirable air and charcoal (namely, carbonic acid) and quick-lime. Hence, from the principle referred to, it could not be concluded, that carbonic acid, combined with quick-lime, would be decompounded by phosphorus attracting its respirable air, and the phosphoric acid, thus formed, attracting the quick-lime. Experience only could determine the result of these affinities, but no proof has been given, from the examination of the mixture, after applying phosphorus to red hot marble; such as finding that carbonic acid was really decompounded, because there was a deficiency of this elastic fluid, and that the charcoal and phosphoric acid corresponded to this deficiency. Accordingly some chemists have conjectured that the small quantity of charcoal afforded in this experiment pre-existed in the phosphorus, which, it is well known, is distilled from charcoal; and others have suspected that it might have arisen from accidental impurities.

As experience, also, has taught us that phosphorated mineral

alkali will not yield phosphorus by exposure to charcoal and heat, unless *plumbum corneum* be added, we cannot infer that the carbonic acid in mild mineral alkali will be decompounded by phosphorus; because, as in the case of bone-ashes and phosphorus, the joint affinities between respirable air and phosphorus, and between phosphoric acid and mineral alkali, are, by this fact, shown to be *not inferior* to the conjoined affinities between charcoal and respirable air, and between carbonic acid and that alkali. No other conclusion can be drawn with respect to the affinities exerted when charcoal is applied to phosphorated vegetable alkali; because the affinity is stronger between the phosphoric acid and vegetable alkali, than between the same acid and mineral alkali. As the attractive forces between phosphoric acid and barytes, and between that acid and magnesia are, very probably, at least equal to those between phosphoric acid and fixed alkalies, the question, whether carbonic acid united to these earths can be decomposed by phosphorus, remains to be determined by experiments. But with respect to the volatile alkali, we know, by the experience of making phosphorus from urine, that the united affinities between respirable air and phosphorus, and between phosphoric acid and volatile alkali, are *inferior* to the joint affinities between charcoal and respirable air, and carbonic acid and volatile alkali; hence, in a due degree of heat, phosphorus and mild volatile alkali are formed from phosphorated volatile alkali and charcoal, consequently carbonic acid combined with volatile alkali, cannot be decompounded by phosphorus and heat, even if the volatility of that alkali did not, apparently, render it impossible to apply the requisite degree of heat. We know so little of the degree of chemical attraction betwixt clay

and phosphoric acid, that the question, whether carbonic acid united to clay will be decompounded by phosphorus; can only be answered by future experiments.

As I presume that I have made experiments which enable us to draw conclusions concerning the above cases of compound attraction, and which also shew, in several instances, that carbonic acid is decompounded, and affords respirable air, and charcoal; I think it my duty, on a subject so very interesting in the present state of chemistry, to submit them to the consideration of this Society.

Experiments with Phosphorus, applied to mild fossil Alkali.

I began with attempting to decompound carbonic acid in union with fossil alkali, in preference to the same substance combined with quick-lime, because the proportion of this elastic fluid is much greater in mild fossil alkali than in calcareous earth, because the affinity is not so strong between carbonic acid and fixed alkalies, as between carbonic acid and quick-lime, and because the mechanical separation of charcoal from alkalies, and phosphorated alkalies, may be more easily made than of charcoal from calcareous earth and phosphoric selenite. The purest fossil alkali I could procure was employed, from which I had expelled $\frac{67}{100}$ of its weight of water, but none of its carbonic acid.

Into a thick white glass tube, almost one inch wide, three feet and a half in length, coated within nine or ten inches of the open end, were introduced two hundred grains of transparent phosphorus, and eight hundred grains of the above deaquated alkali were pressed down upon them. The tube, thus

charged, was then bent so that the open end might be kept conveniently plunged in quicksilver during the experiment. The coated part of the tube, containing the alkali, excepting two or three inches next to the phosphorus, was gradually heated over a portable furnace till it was red hot, and rather flexible, in which state the part containing the phosphorus was gradually drawn over the fire, and kept red hot twenty minutes. At the beginning of the experiment, quicksilver rose several inches within the tube, and when the coated part grew hot, phosphorus was sublimed into the upper and cool part of it: about twenty drops of water were condensed over the quicksilver; and two ounce measures of phlogisticated air, with a little respirable air, which had the smell of phosphorus, came over. The tube, when cold, being broken, the lower part was found to contain a loosely-cohering solid, as black as charcoal, which weighed 428 grains, and above this, a grey and white substance, partly fused, and partly in a powdery form, which, with adhering glass, weighed 358 grains. Neither in this, nor in other similar experiments, was I able to collect the whole contents of the tube, without glass which had been melted, that adhered to the alkali, on which account I could not determine accurately the total weight, independently of glass; but I was sure, from a number of trials, that it was a little less than the original weight of the alkali. The phosphorus, sublimed into the upper part of the tube, was moist from the adhering phosphoric acid: it was inflamed by slight friction, *viz.* merely on breaking the tube.

The 428 grains of black alkaline matter thus obtained, afforded, by solution in boiling hot concentrated acetous acid, a little more than 25 ounce measures of carbonic acid, under the mean pressure of the atmosphere, and of the temperature of

45°; that is, 100 grains of the black matter yielded about six ounce measures of this elastic fluid. In other similar experiments, the quantity of carbonic acid varied from four to seven ounce measures in 100 grains of this blackened alkali; except in one experiment, which afforded only three ounce measures of the acid, but the largest proportion of charcoal I ever made, namely, 12 grains.

The solution of the above 428 grains was filtered, and the residue, which was black, was lixiviated with boiling distilled water. This residue, when dried, weighed 32,4 grains; it had no taste or smell, but was an impalpably fine, intensely black, and very light powder; for it occupied a one ounce-and-a-half measure, therefore, may be estimated to have been about 22 times lighter than water. A little of this black powder, being thrown upon a red hot iron plate, ignited readily, but left a residue, which was one-fourth of its weight: this being again thrown upon the red hot iron plate, it ignited, and there remained, on cooling, a very small portion of brownish powder, which diminished to almost nothing, by being applied twice more to the iron kept red hot for several minutes. On sprinkling this black powder upon boiling nitre, it sparkled most brilliantly and detonated, leaving a colourless mass entirely soluble in water. This black powder, mixed with powdered nitre, deflagrated on exposure to heat, in a retort with the air-apparatus affixed to it, affording, over quicksilver, carbonic acid. This black matter, also, reduced the calx of lead; being mixed with tartar of vitriol, and heat being applied, *hepar sulphuris* was produced; and with phosphoric acid, phosphorus was obtained. That, therefore, these 32,4 grains were charcoal, cannot, I think, be doubted. I might

add, that accidentally I found this powder, red hot, decomposed water as charcoal does.

The above filtered liquid was evaporated to one pint, and shewed signs of acidity: to it was added muriated lime till it produced no further precipitation. The dried precipitate weighed 130 grains, and was found to be phosphoric acid combined with lime; and the liquor, in which this precipitation took place, was ascertained to be muriated and acetated fossil alkali, with a little redundant acetous acid, and a small portion of phosphoric selenite.

The grey and white alkaline matter, with bits of melted glass, weighing 358 grains, as abovementioned, by solution in concentrated acetous acid, afforded 41 ounce measures of carbonic acid, and a residue upon the filter, which, when well dried, weighed 44 grains. This residuum consisted of rough, sharp-pointed, black and white particles; it was much specifically heavier than the residue of the other part of the alkaline matter abovementioned to have been examined; it deflagrated a little on being thrown upon boiling nitre, but left above $\frac{1}{5}$ of its weight of matter insoluble in water, and which I supposed was vitrified. The filtered liquor from these 358 grains of alkaline substance yielded to the precipitant muriated lime 21 grains of phosphoric selenite.

To satisfy myself still further, that carbonic acid had been destroyed in this experiment, and to form some estimate of the quantity which had disappeared, I separated it, by concentrated acetous acid, from 400 grains of mild alkali, taken from the same parcel as that was which afforded charcoal, and I found the quantity to be 104 ounce measures, or 26 ounce measures in each 100 grains of mild alkali.

To afford a still more decisive proof that carbonic acid had not been combined, or escaped, in this experiment, but had been destroyed, I exposed some of the same parcel of alkali which had afforded charcoal to the same degree of heat, in tubes, under similar circumstances to those in the above experiment; and I found that no carbonic acid, but a little water, came over into the air-apparatus; that the total weight of the alkali was diminished, but that a given weight of it, after the experiment, afforded rather more carbonic acid, by solution in acetous acid, than an equal weight of the same parcel of alkali not thus subjected to heat. This diminution of weight of alkali, and greater proportion of carbonic acid, I impute to the water visibly separated in the glass tubes, and, perhaps, also absorbed in the earthen ones. Accident afforded a still more decisive proof of the decomposition of carbonic acid. In the beginning of the experiment, the tubes sometimes cracked about four or five inches from the part containing the phosphorus: on cooling, I found, in the part below the crack, black alkaline matter, which yielded much less carbonic acid than the same weight of alkali before the experiment; whereas the alkali above the crack was white, and contained the same quantity of this elastic fluid that it did before it was exposed to heat.

In the experiment above particularly described, it appears that in one part of the alkali there was a deficiency of twenty ounce measures of carbonic acid *per cent.* of alkali; but a production of rather more than eight grains of charcoal, and of as much phosphoric acid as formed about thirty grains of phosphoric selenite; the composition of which may be estimated to be, of phosphorus, five grains; respirable air, ten grains; and quick-lime, fifteen grains. Now, as it has been

demonstrated by M. LAVOISIER, that charcoal, either totally, or a minute proportion excepted, combines with respirable air, and forms carbonic acid; and other familiarly known, though less accurate, experiments, show that carbonic acid is formed whenever charcoal and respirable air are applied to each other in a due degree of heat; and as there are no other sources perceivable of respirable air and charcoal in this experiment, it seems to prove decisively that they are derived from the carbonic acid, which is decompounded by the superior affinities between phosphorus and respirable air, and phosphoric acid and alkali, to those between respirable air and charcoal, and carbonic acid and alkali. An additional proof of the reality of this decomposition is afforded by the examination of the 358 grains of white and grey alkaline matter, of the same experiment, which afforded much more carbonic acid, and much less charcoal and phosphoric acid. I am very fully aware that the proportions of respirable air and charcoal, produced in this experiment, do not correspond to the proportions of them we should have expected, consistently with the synthetical experiments concerning carbonic acid. The variation is especially great with respect to respirable air, of which there should have been eighteen grains instead of five, to combine with the whole of the charcoal, but, from the nature of the experiment, we cannot even approximate to the truth with respect to the real quantity of respirable air produced; for the phosphorus which sublimed probably carried off a little of this air, some of the phosphoric acid thus formed fused along with alkali and glass, and some phosphoric selenite remained dissolved in the liquid. Supposing the whole of the charcoal formed in this experiment to be united to respirable air, the quantity of carbonic acid composed may be calculated to be 104

grains; for 32 grains of charcoal combined with 72 of respirable air compose 104 grains of carbonic acid, or 70 ounce measures; to which must be added the 25 ounce measures of undecompounded carbonic acid separated. Then the quantity of this elastic fluid, calculated to be decompounded, and remaining united in about 400 grains of mild fossil alkali, is 95 ounce measures, and the quantity of it actually found to exist in an equal weight of alkali, was about 112 ounce measures: therefore the quantity of charcoal produced does not differ very considerably from that calculated to be contained in the carbonic acid decompounded. But future experiments must determine whether there is a like coincidence with respect to the other supposed constituent of carbonic acid, namely, respirable air.

I deem it unnecessary to relate a number of experiments which I have made, the result of which was similar to the preceding one; but it may be proper to mention, that in every instance, the proportions of phosphoric acid and charcoal were inversely as the quantity of carbonic acid remaining in the alkali; and that the quantities of these two products diminished as the quantity abovementioned of phosphorus was diminished; accordingly, the alkali most exposed to the phosphorus contained the greatest proportion of charcoal.

I made this experiment several times with alkali, which contained a good deal of water, and then I obtained a large quantity of air, which smelt of phosphorus, but did not explode on contact with atmospheric air; it contained no carbonic acid, nor phlogisticated air, excepting a few ounce measures in the first jar that came over, but it exploded loudly when mixed with an equal bulk of dephlogisticated air, on applying a

lighted wax taper. A charge of 95 grains of phosphorus, and 540 grains of the above alkali, afforded 206 ounce measures of this inflammable air, which was of the same quality whether it was received over water or quicksilver. This air, I apprehend, was produced by the decomposition of the water in the alkali, in consequence of the superior affinity between phosphorus and respirable air, to the affinity betwixt respirable and inflammable air. Therefore when moist alkali is used, *cæt. par.* more phosphoric acid will be formed than when dry alkali is employed; and in calculating the quantity of respirable air formed, regard must be paid to the decomposition of water. It appears also, that it requires less heat to decompound water by phosphorus, than to disunite carbonic acid from fixed alkali.

In these experiments I frequently used thick white glass tubes, and applied heat for a long time, to the degree of rendering them flexible: when cold, I found the internal surface in contact with the black alkaline matter full of cells, or small cavities, and rough, to which small grains of lead adhered, consequently the respirable air of the calx of this metal in the glass had been attracted, and reduction effected. This reduction might be produced by three substances here present, namely, phosphorus, inflammable air, and charcoal; but I impute it to the charcoal; 1st. because I obtained no particles of lead by passing phosphorus through a tube filled with powdered white glass, heated to the degree of rendering the tube soft, although on cooling, I found the internal surface of the tube was turned black, which colour could not be removed by rubbing, or by acids. This appearance I cannot explain. 2dly. This reduction is effected when there is no

water present, at least when no inflammable air is extricated. 3dly. The greatest quantity of regulus of lead was obtained in those parts of the alkaline matter which contained the smallest quantity of charcoal, and therefore, I conceive, the charcoal, formed in those parts, had united to the air of the calx after the phosphorus had been driven through the alkali, so that the carbonic acid thus composed could not be decompounded, but was combined with the alkali, which was always redundant. In calculating the proportion of carbonic acid decomposed, it will be necessary to consider the reduction which here takes place.

If the air-apparatus be not affixed to the tube, containing a charge of phosphorus and alkali, charcoal and respirable air will be formed; but the phosphorus will take fire at the open end of the tube, and burn with splendour, as in dephlogisticated air.

Porcelain, or well glazed WEDGWOOD tubes, answer in these experiments better than glass ones, the insides of which are apt to melt; but unglazed vessels allow the phosphorus to pass through their pores, though part of the carbonic acid may be decompounded.

The heat applied must be greater than the white glass now made can endure without melting; for, I passed phosphorus through a tube containing mild fossil alkali, heated so that it appeared red hot in the dark, and no charcoal was formed, though the inside of the tube was blackened.

Experiments with Phosphorus applied to mild vegetable Alkali, calcareous Earth, Barytes, Magnesia Alba, and Clay.

Similar experiments to the preceding, made with mild alkali of tartar, in place of fossil alkali, afforded, apparently, as much charcoal, and which was easily obtained, but as the phænomena were similar, and as I have not ascertained with any tolerable precision the proportion of the carbonic acid decompounded, and of the products, it is unnecessary to give any further account of them.

By the like experiments, I endeavoured to decompound the carbonic acid in calcareous, barytic, magnesian, and argillaceous earths. The matter remaining in the tubes, after exposure to heat, was blackish, and grey, seemingly from charcoal being formed, though in much smaller quantity than in the preceding experiments with fixed alkalies. For the reasons above given, I omit the particulars of these experiments on earths.

It appears to me, that the above experiments justify the inference that the joint affinities between respirable air and phosphorus, and between phosphoric acid and mineral alkali, are superior to the affinity between the whole, or at least part, of the respirable air of carbonic acid and charcoal, cooperating with the affinity between that acid and the same alkali. And, although I have not ascertained the facts with equal satisfaction, the experiments already made seem to warrant the conclusion, that the order of the affinities is such, that carbonic acid united to vegetable alkali, lime, barytes, magnesia, and clay, will be decomposed by phosphorus in a due degree of heat. With respect to carbonic acid combined with

volatile alkali, as might be expected, I could not decompose it, although I transmitted boiling hot phosphorus through a very long tube, containing mild volatile alkali.

Experiments with Phosphorus applied to Quick-lime, and caustic fixed Alkalies.

I need not explain that these experiments must confirm or invalidate the conclusion above drawn, that carbonic acid was decomposed by phosphorus applied to mild alkalies, and earths which contain this elastic fluid.

As the quick-lime which can be procured in London must contain both water and carbonic acid, I exposed a quantity of this earth 48 hours to the fire of a reverberatory furnace, by which it was contracted to half its former bulk, and was diminished in its weight; it was, however, still soluble in acids, and afforded no carbonic acid. In the manner above described, I exposed 240 grains of it, with 60 grains of phosphorus, to heat in a coated glass tube. On breaking the tube, when cold, I found at the bottom about 30 grains of blackish and white powder; and above that, to the extent of four or five inches, was a rose-coloured powder, which by its contact with air soon became of a reddish brown colour; above this was the quick-lime, scarcely altered in its colour, but it had, like the rest of the powder in the tube, an alliaceous smell. On tasting a little of this reddish powder, I was surprised by its exploding upon my tongue. I threw a few grains of it into several ounces of cold water, it did not seemingly dissolve, or turn black, but in a few minutes emitted air-bubbles, which rose to the surface of the water, and then burst and exploded,

producing a white circular cloud, which in ascending expanded gradually, till it burst in the air. It continued to emit these bubbles from time to time, during an hour, and then left a grey sediment, which was phosphoric selenite and lime, and the water tasted strongly of lime. The same powder, in hot water, exploded more rapidly and loudly than in cold, but not so violently as the phosphoric air obtained by boiling phosphorus in a lixivium of caustic fixed alkali. By putting this powder into an inverted jar of water, I collected a quantity of the air which it produced; it had the properties of the phosphoric air already mentioned, and, amongst others, by standing over water a day or two, it became no longer spontaneously inflammable, but appeared to have deposited phosphorus upon the water and sides of the vessel, and exploded upon applying to it a lighted wax taper. This powder, therefore, I apprehend, consists of phosphorus and lime united by heat; it readily decomposes cold water, and then the inflammable air disengaged unites with, or rather suspends, a portion of phosphorus, and forms phosphoric air. The phosphorus thus suspended by standing is deposited, and inflammable air alone remains; the other constituent of water, respirable air, unites to another portion of phosphorus, and composes phosphoric acid, which combines with lime, and forms phosphoric selenite. This compound of lime and phosphorus, which some of my chemical friends have called *fulminating bepar of phosphorus*,* may be used to obtain phosphoric air with much more ease than by the usual method. †

* In the new system of chemistry it will be called *phosphur of lime*.

† Dr. INGENHOUSZ has devised some surprising and beautiful experiments with this substance.

This experiment seems decisive, that the charcoal afforded in the former ones was derived from carbonic acid.

My next experiment was with caustic alkali and phosphorus. The caustic vegetable alkali I employed was blackish, partly from a very small quantity of calx of iron, and partly, I think, from other causes which I do not understand; and I was not able to prepare myself, or obtain from others, fixed caustic alkali in a solid form which was colourless. It also always contained a small quantity of carbonic acid. I introduced into a glass tube 50 grains of phosphorus, and 150 of pulverized caustic vegetable alkali, previously found to contain three ounce measures of carbonic acid in each 100 grains. This charge was exposed to heat, as in the former experiments. On breaking the tube, when cold, the alkaline matter was blacker than before: a little of it thrown into hot water emitted bubbles of phosphoric air, but not in cold water: in rubbing off this alkali from the sides of the tube some pieces of it took fire. I dissolved as much as I could of this black alkaline matter, by pouring boiling water upon it on a filter: a greenish lixivium passed through first, then a less coloured alkaline liquor; and last of all, limpid water. A residue left upon the filter being dried, weighed ten grains; it was a blackish brown, impalpable powder, at least five times specifically heavier than the charcoal obtained in the preceding experiments.

(a) Six grains of this residue upon a thin plate of iron, heated over a candle, burnt with a green and blue flame, emitting a somewhat arsenical odour, and it did not remain ignited after the flame ceased. A coal-like matter was left, which weighed three grains.

(b) These three grains *(a)* being placed upon an iron plate, red hot, again emitted a little green and blue flame, with the like, but a weaker smell than before; the substance remaining continued ignited but a few seconds of time, though the iron was red hot much longer. The residuum, which was black, weighed two grains and a half.

(c) The residuum *(b)* was exposed in a silver spoon red hot; it soon ignited, and sparkled; but though this heat was applied six minutes, on cooling, a blackish matter remained, which weighed 1,3 grains.

(d) The 1,3 grains of residue *(c)*, under the flame applied with the blow-pipe, gave some indications of fusion, yet it remained black: but

(e) Being thrown into boiling nitre, a slight detonation ensued; this salt was not coloured by it, and it was dissolved in water, leaving scarcely a visible quantity of matter upon the filter.

I think I may safely conclude that but a small part of these ten grains of residue was charcoal: and as the proportion is so much smaller than has been shown to be afforded by an equal weight of alkali saturated with carbonic acid, this experiment confirms the conclusion, that the charcoal produced in the preceding experiments is from the decomposition of that elastic fluid. The small quantity of charcoal in the above ten grains of residuum was, perhaps, intimately mixed with phosphorus and alkali; but more experiments are required to determine satisfactorily the nature of this compound.

To corroborate the inference concerning the source of the charcoal above described, I add, that not a grain of it was produced by applying phosphorus to vegetable alkali and fossil alkali saturated with vitriolic and marine acid.

The resemblance betwixt phosphorus and sulphur, induced me to consider whether carbonic acid, combined with alkalies and earths, might not be decompounded by sulphur. Experience, however, did not favour the supposition of a decomposition in these instances; for it is well known that hepar may be formed by applying charcoal to tartar of vitriol, GLAUBER'S salt, vitriolic selenite, and ponderous spar: and therefore that the affinity between charcoal and respirable air is superior to the joint affinities between respirable air and sulphur, and between vitriolic acid and fixed alkalies, lime, and barytes; consequently, if sulphur be applied to carbonic acid, united to these alkalies and earths, the affinity between sulphur and respirable air cannot disengage charcoal from the carbonic acid in mild alkalies and absorbent earths. This conclusion would, however, only be just, provided no other agents interfered; and as we cannot be absolutely certain that they could not, I repeated the above experiments with sulphur instead of phosphorus; by which I produced a blackish powder, that had the properties of reducing calx of lead, and changing vitriolic salts into hepars: but as it did not burn on red hot iron, and deflagrate with nitre, I cannot pronounce it to be charcoal; thinking it most prudent to reserve this matter for future examination.

Leicester-Square, September 20, 1792.

P.S. In consequence of some observations published in the *Annales de Chimie*, Juin, 1792, Tome XIII. by Mr. FOURCROY, it is thought proper to add, that although the above paper was not read till May last, it was presented to the Society in March

preceding; and the experiments were made during the author's autumnal course of lectures in 1791. In these experiments he was assisted by Mr. EGGINTON, of Queen's College, Cambridge, who attended that course. The products were shown, and the experiments mentioned to several Members of the Royal Society the beginning of last winter, particularly to the President, who honoured the author with his attendance during several of the processes.

The substance produced by M. RAYMOND, referred to in the *Annales de Chimie*, is a *humid* combination of phosphorus and lime, and does not decompound cold water; it is therefore a very different composition from the *phosphur of lime* described in the above paper.

XVI. *Observations on the Atmospheres of Venus and the Moon, their respective Densities, perpendicular Heights, and the Twilight occasioned by them.* *By* John Jerome Schroeter, *Esq. of* Lilienthal, *in the Dutchy of* Bremen. *Translated from the* German.

Read May 24, 1792.

On the Atmosphere of Venus.

ALTHOUGH the evidence afforded us by the most recent observations and discoveries, not only of the existence, but also of the various and singular properties of the atmospheres of Saturn, Jupiter, Mars, and the Moon, be in a manner incontrovertible; yet so inconclusive are the few observations hitherto made on the atmosphere of Venus, that several of the greatest astronomers* have lately thought themselves authorized to doubt its very existence.

The following inquiry may hence prove the more acceptable to those who favour the investigations of nature, not only as it evinces, in my opinion, the existence of such an atmosphere, but also as it points out several inferences concerning its nature and properties, which to me appear new and interesting.

What convinced me, twelve years ago, when I first began to observe Venus with a good three-feet achromatic telescope,

* See *Astronomie de* M. DE LA LANDE, §. 2272.

that it actually has an atmosphere of no small extent, was the striking diminution of light which I noticed on the planet in its various phases from its exterior limb towards the interior edge of its illuminated surface, and especially near the latter: and this appearance it was which induced me to make farther observations on the subject, especially as I found that the phænomenon recurred as often as I looked at the planet with an HERSCHELLEAN four and seven-feet reflector, armed with the higher magnifying powers.

The great number of observations I have now made on this object, for a series of years, being upon the whole very similar in their nature and results, it would no doubt be not only tedious, but also superfluous, to describe them here at length: but the following general remarks it may be necessary to premise, in order to obviate all misapprehension, and the false conclusions that might be deduced from hasty and inaccurate observations.

In fig. 1. Tab. VI. which I have annexed for the sake of greater perspicuity, the light appears strongest at the outward limb *a*, *b*, *c*, from whence it decreases gradually, and in a regular progression towards the interior edge or terminator, and this not only towards its middle *d*, *e*, but also near the two cusps *f g*, *h i*, the light becoming so dim immediately at this border *g*, *f*, *d*, *e*, *b*, *i*, that it commonly loses itself in a faint bluish grey, forming a very indefinite ragged margin, scarce discernible with the best telescopes, and which, in several of the phases, resembles the interior uneven border of the moon, as it appears to the naked eye, or to a power magnifying from 1 to 4 times. The following circumstances must necessarily be attended to respecting these observations.

1. This diminution of light naturally shews itself between the greater eastern and western elongations and the inferior conjunction, when Venus appears about half full, or falcated, but still of sufficient breadth.

2. But this diminution is not equally sensible in all the phases of the planet, but sometimes much more so than at others. These appearances depend greatly on the clearness of our atmosphere, on the telescope giving a very distinct, soft, mellow image, and on the eye being properly prepared for such observations.

3. When, in the more falcated phases, we wish to observe distinctly and accurately this diminution of light at the farther extremities of the cusps, it is necessary to advert that, whilst about the middle of the terminating border between *d*, *e*, where the luminous part has its greatest breadth, this diminution, if seen under favourable circumstances, is too perceptible to leave the least doubt or suspicion, the points of the cusps, especially when the weather and other collateral circumstances are not favourable, appear, indeed, somewhat fainter, but yet almost as bright as the outward limb near *b*. As the diminution of light, whether the cause proceed from its own nature, or from that of the atmosphere, must be in the same proportion at the cusps as at the middle of the terminator, I could not but be puzzled at this appearance; but the multitude of observations I have since made on the points of the cusps, sometimes when they were tolerably bright, and at others when they appeared as faint as the other parts of the said border, at length cleared up my doubts, and led me to the true cause of this variable appearance, which, to prevent similar perplexities in others, I shall here state at length.

Considering the immense height of the mountains, and the great inequalities on the surface of Venus,* it is natural to suppose that, as at the times of its greatest elongations, one cusp frequently appears pointed and the other blunt, owing to the shadow of some mountain darkening the extremity of the latter, the same appearance may often take place in the falcated phase of the planet. But the cusp whose extremity is covered by a shadow, will, in this case, so far from appearing blunt, always exhibit a pointed appearance; it being in that aspect too sharp for us to distinguish accurately its true form. Should, for instance, the point of the cusp $\alpha\,\beta\,\gamma$, fig. 2, be immersed in a shadow from α to β, this point, in such a case, though it may be somewhat blunted from α to γ, will nevertheless appear sharp, because its whole breadth does often not exceed $\frac{1}{4}$ of a second, whence its true form must needs escape our observation. We do not, in this instance, see the very faintly illuminated point $\alpha\,\beta$, nor yet the real luminous edge $\gamma\,\beta$, but only its prominent and brighter part $\alpha\,\gamma$; a deception this, which cannot take place in the other parts of the terminator, where indeed the same causes, *viz.* the shadows of mountains, will, no doubt, at times occasion an uneven, ragged appearance, but cannot materially affect the very faint light of the whole.

It is, moreover, evident, that even when the extremities of the cusps are, as in fig. 1, not darkened by shadows, the faint luminous points $f\,g$, $b\,i$, are not always visible, but can only

* The perpendicular height of these mountains, as I have already shewn in a former work, is in proportion to the diameter of the planet, at least as great as the height of the mountains in the moon, in proportion to its much shorter diameter; this fact I have had frequent opportunities to ascertain. See Selenotop. Fragm. §. 522.

be seen with certainty at such times when the air is very clear and calm, and a good telescope with a large aperture, and high magnifying power, is used in the observation. And it follows that, except under those circumstances, the parts of the cusps *a f*, *c b*, must, on account of their great sharpness, be the apparent and sufficiently luminous extremities; since, according to well known principles, the exterior points *a*, and *c* will have as much light as the point *k*, near the middle of the crescent.

But in a clear and calm atmosphere, and with a high magnifying power, it is truly pleasing to see, after the eye is accustomed to it, how the whole of the terminating border, even to the farther extremities of the cusps, vanishes gradually, and becomes at last so faint, that in the day time, and where there are any inequalities, it insensibly loses itself in the colour of the sky. Such striking diminutions of light have I seen repeatedly with my four-feet reflector, with a power magnifying 280 times, and my seven-feet reflector, with a 370 magnifying power; and particularly on the 20th of November, 1791, when with a power of 161, I saw the light of the terminator dwindle away, and appearing, for a breadth of about 1 or $1\frac{1}{2}$ seconds, almost as grey as the ash-coloured spots on the moon.

Those who are at all acquainted with the theory of light, need hardly be reminded that on an illuminated spherical surface of a planet, the light will ever appear fainter towards its border, in proportion as the angle between the incident ray from the sun and the said surface becomes smaller.*

* It may equally be superfluous to illustrate how, since the sun appears on the planet Venus under a diameter of about 44′, its luminous border must have a no inconsiderable penumbra. It appears, however, from calculation, that this whole pe-

But what here claims our particular notice, is the singular circumstance that, except in the planet Mercury, concerning which I have not yet a sufficient number of observations, a similar diminution of light is not observed in so sensible a degree on any of the other planets of our solar system, our earth only excepted.

Mars, Jupiter, and Saturn, cannot, indeed, on account of their great distance, exhibit on our globe the variable phases of a half, or smaller portion, of an illuminated hemisphere, whence no fair arguments can be derived from those instances: but the comparative appearances of the moon, in this respect, will be thought the more singular if carefully attended to, the marginal diminution of light on this satellite, which, however, like Venus, is a sphere illuminated by the sun, not being nearly so perceptible and evident, as that above described. Of this we may fully persuade ourselves, by comparing the appearances of the terminating borders of the moon in its falcated phases or quadratures, with the same borders on Venus at the same periodical aspects. Should this striking difference not be reconcileable upon our established optical principles, nothing will remain but the analogy, that, as the density of our atmosphere checks the sun-beams the more, the longer they proceed therein in a direction which, after the rise or before the setting of the sun, carries them over a certain track of land; and as such a track, on which the sun at its rising and

numbra, which, according to the greatest apparent diameter of Venus, extends from 59 to 60″, measures in the direction perpendicular to the line of the cusps only 0,36″, or $\frac{1}{5}$ of a second of a degree; and that where this penumbra is observable in its exterior darker parts, its breadth is not above $\frac{1}{15}$ of a second, whence it cannot be visible throughout.

setting shines from the horizon, is but feebly illuminated; so also is Venus circumstanced with regard to the light it receives from the sun. To compare with some accuracy the intensity of light at the terminating borders on our earth, relatively to its full perpendicular illumination, with the same phænomenon on Venus, may, for want of opportunities to observe both planets at once from a proper distance, as may be done with regard to Venus and the moon, not be very feasible; but this, I think, I may with great confidence infer from my long series of observations, that Venus has an atmosphere in some respects similar to that of our earth, and which must far exceed that of the moon in its density, or power to weaken the rays of the sun.

Thus far had I proceeded in my observations, when the heavens favoured me with the following ones, which may prove the more interesting, as they not only confirm those hitherto made, but also lead to some farther inferences concerning the atmosphere of Venus.

Among all the favourable circumstances for observing the planet Venus, none could be more so than those I had in the months of March and April, but especially from the 9th to the 16th of March, 1790, when, besides the almost constant and unusual serenity of the sky, the planet, which was then in Aries, at 7° and 8′ N. declination, was so high above the horizon, that, notwithstanding its approaching inferior conjunction on the 18th of March, at 4 P. M. I could still view it on the 16th, and should certainly have observed it during the conjunction, had not the weather become hazy on the 17th.

Under these very fortunate circumstances, I fell accidentally (not having, after ten years of constant attention to Venus,

been able to devise any new mode of observing) on the 9th, 10th, 11th, and 12th of March, upon an observation, which I repeatedly confirmed, and which, on account of its singularity, and the light it will probably throw on the physical constitution of this planet, will certainly be ever thought important; especially as it may not in many years be repeated under so favourable a combination of incidents.

On the 9th of March, 1790, immediately after sun-set, and till $6^h\ 45'$, I saw Venus with a seven-feet reflector, magnifying 74.95, and 161 times, very distinctly, and uncommonly splendid. The southern cusp did not appear precisely of its usual circular form, but rather as is represented at *a*, fig. 3, inflected in the shape of a hook beyond the luminous semicircle into the dark hemisphere of the planet. This, however, after my former observations,* was not new to me: but a far more striking phænomenon, which I had never seen before, excited my admiration, and particular attention. The northern cusp was terminated at *b*, in the same narrow tapering manner as the southern, but did not extend in its bright luminous state into the dark hemisphere. From its point, however, the light of which, though gradually fading, was yet of sufficient brightness, a streak of glimmering bluish light proceeded into the dark hemisphere, from *b* to *c*, which though intermittent as to intensity, was yet permanent as to duration, and although very faint, could yet be plainly seen with both the abovementioned magnifying powers. Like the luminous line then seen on Saturn, its light seemed to twinkle in various detached points, and appeared throughout not only very faint, when compared with the light at the point of the cusp, but also of

* See Selen. Fragm. §. 523.

a very peculiar kind of faintness, verging towards a pale greyish hue.

The limb of the planet at this small part of its dark moiety, appeared with as faint a light, and, compared with the extremity of the southern cusp, as pale as the dark limb of the moon three days before and after the new moon, when it is faintly illuminated by the reflected rays from the earth: and it appeared to me, that towards the farther extremity *c*, where it was actually inflected, according to the circular limb of the dark hemisphere, its light vanished into a pale bluish tint, in the same manner as the more vivid light of the luminous hemisphere dwindles away towards the terminating border and the extremities of the cusps. The 3d fig. represents this striking phænomenon only in its projection, it being impossible to give a just representation of it in a drawing. Surprised at this new and singular appearance, I pointed it out to Mr. TISCHBEIN, an ingenious artist, who was then with me engraving the plates for my Selenotopographic Fragments, and desired him to attend particularly to all its circumstances.

He found and described the whole just as I had seen it. I made the drawing, fig. 3, under his eye; and we viewed the object together very attentively for about an hour, when the planet descended too low to be distinctly seen.

The apparent diameter of Venus was, by means of the projecting table, and the mean of several observations, all of which agreed to within 1″, found to measure 59″; but the greatest breadth of the illuminated part did not exceed 2,6″.

On the following evening, the air being as calm and serene as the preceding one, I observed the planet from 6^h to $6^h\,40'$, but on account of some necessary alterations in the apparatus

of the seven-feet reflector, I could use only the four-feet instrument, with powers magnifying 134 and 70 times. The southern cusp had its luminous prolongation, but not quite so distinct as the preceding night; but what was more remarkable, each cusp, but chiefly the northern one, had now most evidently a faint tapering prolongation, of a bluish grey cast, which, gradually fading, extended along the dark hemisphere, so that the luminous part of the limb was considerably more than a semicircle.

On the next night, being the 11th of March, when the seven-feet reflector was again fit for use, I found Venus before sun-set, with a power magnifying 95 times. At six o'clock, I saw distinctly the southern point terminating in a luminous streak; which now, as in the evening of the 9th, was longer and narrower than the bright termination of the northern cusp. The apparent diameter of Venus measured 60″; but the greatest breadth of the illuminated part could not be ascertained on account of some undulations in the air, occasioned by rising vapours: the light, however, sparkled through this tremulous medium, similar to the flash in a silver furnace; and comparing what I could distinguish with the table of projections, this breadth I found could not well exceed 2″. I also saw most distinctly the faint bluish luminous streak, sensibly extending in an inflected direction beyond the bright semicircle, and continued observing it till 6^h 30″, when, on account of the descent of the planet, I ceased my observation. The powers I used magnified 95 and 74 times.

No doubt can hence be entertained of the reality of this memorable phænomenon; but for reasons which I shall give hereafter, I was very solicitous to be able to measure the pro-

jection of this glimmering strip of pale light, and to ascertain whether in fact it only belongs to the northern cusp.

The very next, or the fourth evening, gave me a favourable opportunity for this purpose, and this was the more fortunate, as, on account of the hazy weather that immediately succeeded, of the increased twilight into which the planet advanced, and of its decreasing declination, this was the last evening in which such an observation could be made with some precision. At six o'clock, the atmosphere being uncommonly clear, I looked at Venus with the seven-feet reflector, magnifying 95 and 74 times. It appeared very distinct, and I ascertained, beyond the possibility of doubt, that the southern cusp projected somewhat into the dark hemisphere, and that from the point of the northern one, the very faint narrow streak of pale bluish light, intermittent in intensity on account of its faintness, but yet permanent as to duration, extended several degrees along the limb of the dark hemisphere of the planet. I strained all my visual powers, but could actually see this appearance only at the northern point. As the planet descended towards the horizon, the light of the very sharp southern, as well as that of the bright part of the northern point began to glimmer with gradual diminution, the latter more faintly than the former; but not the least appearance of a pale bluish prolongation could be traced at the southern cusp.

I had immediate recourse to the projection machine, and though I found it very difficult to measure, with the greatest accuracy, the very faint light at the northern, as well as the bright prolongation at the southern cusp, I succeeded, however, to ascertain

1. That the faint streak at the northern cusp, as represented in its projection, fig. 3. extended at least 8″ of a degree along the limb of the dark hemisphere.

2. That the prolongation of the southern cusp measured likewise full 8″, its inflected hooked form appearing now very distinctly: and,

3. That the apparent diameter of Venus subtended an angle of between 59 and 60″.

Had it not been for these very favourable observations from the 9th to the 12th of March, 1790, which evinced, beyond a possibility of doubt, that the illuminated limb of Venus actually exceeds the semicircle, I should, perhaps, never have arrived at the important cosmological discovery it led to; the days preceding them being too hazy, and those following, though somewhat clearer, yet too near the inferior conjunction of the planet to admit of fair observations. On the 17th, and on the 18th, when the conjunction took place at 4 P. M. the sky was cloudy. From the 21st to the 25th, as also on the 30th of March, the 1st, 7th, 8th, and 21st of April, the mornings about five o'clock were indeed sufficiently clear, and it may easily be imagined, that, considering the importance of the discovery, I did not fail to avail myself of them; but the northern declination of the planet had now decreased too much, and on the other hand, the twilight in our latitude was become too great to see distinctly and with certainty what I had observed before. I had no doubt, at times, of the prolongation of either cusp into the dark hemisphere; but circumstanced as the objects were, I was much upon my guard against visual deceptions. My journal, however, supplies me with the following appearances:

1. March 23d, 5^h 10′, $4\frac{1}{2}$ days after the conjunction, a faint light appeared, at times, playing along the dark limb from the southern cusp. The arc appeared hooked and irregular.

2. March the 24th, 5^h. I saw the exterior limb of the illuminated part manifestly much more convex at *a*, fig. 4. than at *b*. I also actually saw, at present, near *d*, a grey, faint, twinkling light, proceeding from the southern cusp in an inflected direction. Something of a similar appearance was likewise seen at the northern cusp *c*; but this was still fainter, and somewhat less inflected. The bright extremities of the cusps were very sharp, and similar in point of illumination.

3. March 25th, 5^h 15′. The bright hooked prolongations extending from both cusps, were not so perceptible as on the preceding days; but I was now the more struck on seeing, under the present circumstances, manifest traces of the same faint ash-coloured inflected streaks extending into the dark hemisphere, which I had beheld on the 10th of March, and which now proceeded from the extremities of both cusps, but more perceptibly so from the southern. See fig. 5.

4. March 30th, $11\frac{1}{2}$ days after the conjunction. I now saw no more of these appearances than I had done on the 9th and the preceding days of this month, particularly the 2d and 5th, when nothing of this phænomenon could be perceived.

As these observations, made after the conjunction, fully confirm those from the 9th to the 12th of March, I shall now proceed to state the inferences that appear to me evidently to result from them.

As there can now remain no doubt of the appearance of the pale ash-coloured streak of light, *b*, *c*, fig. 3. extending along the limb of the dark hemisphere of Venus; and as this planet

cannot be said, like the moon, to receive some light upon its dark side from our earth, or any other heavenly body, it follows that this light must either proceed immediately from the sun, which, as I have frequently observed concerning the high mountains Leibnitz and Doerfel in the moon,* throws its rays directly on the tops of very lofty ridges of mountains; or else that it is a light which partly illuminates the atmosphere of Venus, and partly, being reflected by this atmosphere, marks out by a faint glimmer the limb of the dark hemisphere of the planet, in the same manner as our morning and evening twilight acts upon ours.

All our present observations militate against the supposition of this phænomenon being the effect of light immediately proceeding from the sun; for,

1. This light does not appear, as on the mountains Leibnitz and Doerfel in the moon, in single, detached, and distant points; but as a continued streak of light, proceeding from the extremities of the cusps, and continuing along the limb of the dark hemisphere to a distance of about 8″, or, in proportion to the apparent diameter of the planet, no less than 15° 19′ of its circumference. This light also, compared with the bright part of the phase, is not unlike the comparatively pale limb of the dark part of the moon before and after its conjunction.

2. Were this the light of the illuminated summits of a chain of mountains, it would not appear so even, regularly connected, and spherical, as we behold it. But what removes all manner of doubt is,

3. The very different, extremely faint, bluish-ash-coloured appearance of this glimmering light, which forms as great a

* See Selen. Frag. § 75. and Tab. IV. fig. 6 and 8.

contrast with the whitish more vivid light which is seen immediately on the cusps, as the ash-coloured light reflected from our earth on the dark limb of the moon does, when compared with the solar light on its phase. This pale light in the dark hemisphere, it must be owned, faded away towards its termination, in the same manner as the solar light did at the edge of the bright phase: but had this faint streak, like the more vivid light, been an immediate emanation from the sun, the gradual diminution would have been throughout progressive in a continued proportion; and the light in the dark part, immediately contiguous to the points of the cusps, must have had nearly the same degree of brightness as the points themselves, which was by no means the case.

Every circumstance, therefore, seems to evince that this phænomenon is occasioned by a light reflected by the atmosphere of Venus into the dark hemisphere of the planet, being in some measure the light of the atmosphere itself, when illuminated by the rays of the sun, or, in fact, a real twilight. But this will appear still more manifest when

4. We compare, according to the abovementioned observations, the alternate relative appearances of the cusps of Venus reciprocally with each other. On the 9th and 12th of March, 1790, when the southern cusp extended, not in the true spherical curve of the limb of the planet, but in a somewhat hooked direction, into the dark hemisphere, the pale bluish ash-coloured streak appeared only at the point of the northern cusp, from whence it proceeded, in a true spherical curve, along the dark limb of the planet. On the 10th of March, on the other hand, when the southern cusp did not penetrate so far into the dark hemisphere, the pale streak was perceived at both

points, though somewhat more sensibly at the northern than at the southern; and such also were the appearances after the inferior conjunction. These appearances will be thus explained by the effects of a twilight. The bright prolongation of the southern cusp, as it was seen on the 10th and 12th of March, *a*, fig. 3, must be ascribed to the solar light illuminating a high ridge of mountains situated at this region, whence it appears why this prolongation might not be strictly spherical. The twilight that must have existed at this part, would naturally be eclipsed by the much greater brightness of the light immediately derived from the sun, in the same manner as, on our earth, mountains that face the rising or setting sun, are known to darken the twilight that ought faintly to illuminate the regions situated immediately behind them. Were this not the case, there is no doubt but that a true spherical arch of the limb of the dark hemisphere would have appeared faintly illuminated: and such did we see was the effect of this twilight produced at both cusps when the bright prolongation was not considerable, the appearance being most decisive at that cusp where there was least of this prolongation.

Should this pale streak of bluish ash-coloured light actually be the effect of a twilight, it will not only prove the existence of an atmosphere, but we shall also, from the length of the streak, as measured on the 12th of March, 1790, be able to draw some inferences concerning its density, and real extent on the surface of the planet.

According to that observation, the apparent diameter of the planet measured 60″, and the visible stretch of the twilight, as far as it could be seen with a power of 95, applied to a

seven-feet reflector, was estimated at 8″, or eight of those parts of which 60 go to the diameter. This gives a circumference of 188,4, and the degree will be equal to 0,52 of these parts: and taking those 8″ as a chord, the arc, *b c*, fig. 3, over which the twilight extends, will measure 15° 19′; this being the portion of a great circle over which, in circumstances as favourable as those I was favoured with, the twilight will be seen extending over the dark hemisphere of the planet. This, and no more, is what I meant to express in one of my letters in which I mentioned the phænomenon in general, without any accurate computation, or particular induction.

But this pale luminous arc, *b c*, fig. 3, as it appears on the planet, not when it is at its greatest elongation, when we see half its illuminated surface, but at a time when it is approaching very near to its inferior conjunction, is in fact only the apparent, but by no means the real extent of the twilight, in a perpendicular direction east and west from the circle terminating the dark and illuminated hemispheres.

This real distance must evidently fall much short of the length of the abovementioned arc. The following is the method I have adopted to deduce the real extent of the twilight of Venus, from the apparent one as seen at the points of the cusps.

Let S, fig. 6, represent the sun, A D C B F the orbit of Venus, and T the place of the earth relatively to every aspect of Venus in its orbit. B will then be the point of inferior conjunction, C of the greatest western elongation, D of the superior conjunction, A of the greatest eastern elongation, and F nearly the point, not far distant from the inferior conjunction,

at which I observed the planet on the 12th of March, 1792, when I measured the extent of its twilight.

Let also A B G C fig. 7, be the hemisphere of the planet, turned towards our earth T, and A F D E C the other hemisphere turned towards the sun S, at the time of an inferior conjunction. And let A C represent the terminating surface perpendicular to the plane A F D E C G B extending from the centre of the sun through that of the planet Venus, and the eye of the observer on our earth. A *f c* C will then be the mutual edge of the two hemispheres, and the whole of the dark one A B G C will be directed towards the eye of the observer at T.

But whenever Venus, before or after an inferior conjunction, comes into such a position, relatively with the sun and our earth, as to be illuminated by the former in the direction *b* E *c*, fig. 7, which position, on the 20th of March, 1790, must have been nearly at F fig. 6; then will F *g c* G, fig. 7, be the mutual terminating surface of the dark and light hemispheres of the luminous circumference; of which surface, however, only one half, *c* G, can be seen from the earth; the other half, F *g c*, lying in the hemisphere that is turned away from the eye: *c* will be the point of the cusp seen from T.

This being premised, let the twilight of Venus be so considerable as to extend, in a gradually-fading progression, from the luminous terminating border F *g c* G, to the line *b f k i*, which let us assume as the real border of the twilight. Then will a part of it, K *f*, terminating at *f* in the direction *a g*, parallel to *b* E *c*, the central ray from the sun, be seen projecting into the dark hemisphere of the planet, and this prolongation, as it was seen on the 12th of March, 1790, will be the appa-

rent extent of the twilight; but its true extent will only be the distance $f\,g$ measured in degrees of a great circle. And it is sufficiently evident, that the smaller we assume the angle D *c* E, before or after an inferior conjunction, the more pointed of course will be its equal angle A *c* F, and hence also the longer the visible part of the twilight $f\,c$. It is likewise manifest, that when the elongation of the planet is equal to the angle D *c* C, the line D *c k* B will represent the terminating surface of the dark and light hemispheres, whence the planet will, at its greatest elongation, be illuminated by the sun in the direction C *c*, and the true extent of the twilight will coincide with the apparent one on the terminating plane A *c*.

In order then to calculate in any, except the greatest, elongation, the arc $f\,g$, in the right-angled spherical triangle $f\,c\,g$, or to ascertain the true extent of the twilight in degrees of a great circle, it will be requisite to measure from T, the apparent prolongation of the twilight into the dark hemisphere $f\,c$, and the apparent diameter of the planet. With these *data* it follows that, as the angle $f\,g\,c$ is a right angle, the angle $f\,c\,g$ = A *c* F = D *c* E may be found by calculation, and the arc $f\,g$ be thence easily deduced.

But the angle $f\,c\,g$ = D *c* E is equal to the complement of the angle S F T, fig. 6, on the planet, to 180°, or F S T + S T F, as is plainly illustrated by fig. 10, Tab. VII. Let S be the sun, T our earth, *c* the centre of Venus; draw $g\,c$ G perpendicular to S *c* K, and A *c* C perpendicular to T *c*: then is A *c* the projection of the visible terminating plane, as seen from T, and G g the projection of the luminous border. S *c* T is the angle on the planet, K *c* B its complement to 180°, and K *c* B = C *c* G;

For, $E\,c\,G = A\,c\,B = 90°$

$E\,c\,C = A\,c\,K$

And hence $C\,c\,G = K\,c\,B$.

To find, therefore, the angle $S\,c\,T$, calculate for the 12th of March, 1790, at six o'clock in the evening.

The true heliocentric long. of Venus	5°	18°	37′ 35″
Long. of the earth	5	22	21 20
Difference	3	43	45
Heliocentric lat. of Venus	3	23	0
Log. sin. 3° 43′ 45″	=	L. 9.9990795	
+ Log. cos. 3 23 0	=	L. 9.9992424	
Total = cos. $c\,S\,T$	=	9.9983219	
	=	L. 5° 2′ 0″	

Now in order to find the angle $S\,c\,T$, according to this heliocentric distance of Venus from the earth, we have in the triangle $S\,c\,T$, the angle at S, and the two sides $c\,S$ and $T\,S$, being the distances of Venus and our earth from the sun. We then find the log. $c\,S = 4857022$, and the log. $T\,S = 4997758$, and the angle at S being $= 5° 2′$; the sum of the two angles will be $= 174° 58′$, the half of their sum $= 87° 29′$, and then the computation will be as follows:

Log. T S = 4997758
Log. *c* S = 4857022

Log. tang. 10,140736
belongs to 54° 7′ 30″
Subtract 45

9° 7′ 30″
Log. tang. 9° 7′ 30″ = 9205803
Log. tang. 87° 29′ = 11357018

Log. tang. semid. 10,562821
Half the difference of the angles = 74° 41′ 15″
Half the sum = 87 29 0

Angle S *c* T = 162° 10′ 15″
Angle *c* T S = 12 47 45

Now the angle at Venus being = 162° 10′ 15″. Its complement will be = 17° 49′ 45″. But this is = the angle C *c* G = *f c g*, fig. 7, Tab. VI. and hence we farther derive the sine of the arc *f g* = sin. *f c* × sin. *f c g*; whence

Log. sin. 15° 19′ 0″ = 9,4218566
+ Log. sin. 17 49 45 = 9,4859768

Log. sin. *f g* = 8,9078334
which gives *f g* = 4° 38′ 30″.

Such, therefore, is the length of the arc of a great circle of Venus over which its twilight extends, as deduced from its appearance to the inhabitants of our globe under the most favourable circumstances: and if the true semi-diameter of the

planet be assumed = 834 geographical miles, then will the extent of the twilight, in the perpendicular direction from the terminating border, amount to 67 of those miles: and this extent of twilight on Venus, which appears to us to measure $4^{\circ}\,38'\frac{1}{2}$, may, with great propriety, be compared with what LAMBERT calls the common twilight on our globe, when its border passes immediately through the zenith, at a time when he estimates the place of the sun at $6^{\circ}\,23'\frac{1}{2}$ below our horizon.*

This computation affords also the following conclusion: according to the measurement of the 12th of March, 1790, the projection of the twilight, or fc, fig. 7, amounted in its whole length to 8″; whence sin. $fc \times$ sin. $17^{\circ}\,49'\,45''$, gives for the true extent of the twilight fc only 2,45 seconds of a degree; and hence it appears,

1. Why this twilight appears only at the points of the cusps and not at other parts of the terminating border, its extent, with its proportionably faint light, being too small; and this narrow faint light being, moreover, eclipsed by the superior brightness of the luminous hemisphere.

2. Why this twilight is distinctly seen only a few days before and after the inferior conjunction; the visible light of the luminous hemisphere being then only 2″ of a degree in breadth, and only the faint, declining, less luminous part of it apparent to the eye; whereas the projection of the twilight fc, fig. 7, will in this position be most extensive, and hence the more perceptible.

3. But should Venus at this time not be in the sign Aries, nor at so high a declination as when I saw it in March, 1790,

* LAMBERT's Photom. 4. 998, 999. Berl. Ephem. for 1776.

so that the sun do not rise and set much earlier and later than the planet; and should, at the same time, our own crepuscular light be very great; should the planet, I say, when the sun is some degrees below our horizon, have no great altitude, it will then be either not seen at all, or only appear in our strong twilight at or near the horizon. As, even besides all these favourable circumstances, it is moreover requisite that the atmosphere be uncommonly clear, it cannot be thought extraordinary that this very instructive phænomenon, on which I never reflect without particular satisfaction, should not have been seen by me sooner in the long and uninterrupted series of observations I have made, but it will rather be allowed to have been a most fortunate combination of incidents which enabled me to observe it for four days successively. It will also appear,

4. Why this phænomenon did not shew itself again after the conjunction with the same degree of splendour it did before; the northern declination of the planet being then daily decreasing, and the length and brightness of our twilight as gradually increasing. This will also point out the reason why it may not be seen again for some time. The fact, however, being now known to exist, there is no doubt but that some traces of it may, and I flatter myself will, be perceived, even under other perhaps less favourable circumstances.

This discovery, lastly, throws also some light on the constitution of the atmosphere of Venus, and the physical construction of that planet.

It appears, from the abovementioned observations, that however faint the last perceptible degree of evanescent light be in this phænomenon, we do not however, even with the

keenest eyesight, aided by the very best telescopes, distinguish the final termination of the twilight, but can only ascertain the border where its most luminous part begins to fade. If it be then mathematically demonstrated that this more luminous part, which may in some respects be compared to our own twilight, does actually, a few possible but trivial errors in the measurement excepted, extend 4° 38′ 30″ into the dark hemisphere, there can be no doubt but that this twilight does, in its whole extent, approach nearly to the extent of ours, which has been estimated at 18°; and that there is hence a material similarity between the two.

This may be inferred from the uncommon brightness of the solar light on this planet, and the comparatively very bright appearance of the more luminous measured part of its twilight, which also accounts for our seeing this more luminous part so distinctly even in the midst of our own twilight, and perhaps points out why even the pale bluish-grey light which in this, as well as in other proper phases, is seen to terminate the luminous part, forming an evanescent margin of 1 or 1½ seconds, may afford some indication of a twilight gradually blended with the luminous terminating border. When this twilight was measured on the 12th of March, 1790, soon after six P. M. about twenty-six minutes after sun-set, the sun was at most only 4° below our horizon, and the angle *c* T S, fig. 10. Tab. VII. could, according to the above calculations, not measure above 12° 47′ 45″.

Hence it appears that, under such circumstances, the evanescent margin at the termination of the brighter visible part of the twilight of Venus, was at least as luminous as our own crepuscular atmosphere at that point of the heavens. And it

will also be easily perceived, with respect to the much stronger solar light on Venus, that if those last layers of its atmosphere, on which even the least vestige of the solar light can be traced, be transferred in idea into the atmosphere of our earth, admitting a similar density, but a less power of solar light, they would certainly exhibit no such perceptible traces.

And if it be probable, to a degree of evidence, that the twilight on Venus is nearly equal in its extent to that on our globe; if it be mathematically proved that its brighter part, which has so much light as to enable us not only to see it, but even to measure it across our fainter twilight, illuminates a zone 4° 38′ 30″ in breadth, we may hence surely infer the density and perpendicular height of at least the inferior more condensed part of the atmosphere, which is capable of reflecting such a crepuscular light; and apply those principles according to which the height of our own atmosphere, in as far, namely, as its lower strata are able to reflect the solar rays, and occasion a twilight, has been estimated at 34585 toises, or nearly nine geographical miles.

Let *a k b e f l*, fig. 9, Tab. VII. be the surface of Venus; *k b e*, an arc of it, which, according to the above calculation of the extent of the brightest twilight on the planet, is assumed at 4° 38′ 30″; *b d*, the height of that denser part of the atmosphere which actually occasions the twilight that is visible to us; and *b d*, the first ray of the sun, at this time 4° 38′ 30″ below the horizon, which is reflected by the highest stratum of the denser atmosphere *d*, upon the point *e* on the surface of the planet. We then have, in the triangle *d e c*, the side *c e*, or the semidiameter of Venus = 834 geogr. miles, the right angle *e*, and the angle *c* = 9° 19′ 15″, or half the angle

of the twilight. And the side $d\,c$ being to $e\,c$ as rad. is to the cosin. of the angle c, subtract from this $b\,c$, the semidiameter of Venus, and the remainder, $d\,b$, will be the height of the atmosphere. Now, taking 834 geogr. miles = 3178874 toises for the semidiameter, we have

Log.	3178874 = L. 6,5022632	
Log. cos. 2° 19′ 15″ = L. 9,9996486		
Log. $d\,c$	= L. 6,5026196 = 3181400	
	$-b\,c$ = 3178874	
	$d\,b$ = 2526	

Hence it appears that the perpendicular height of the inferior and more dense part of the atmosphere of Venus, which has the power of reflecting the solar light to such a degree, as, under favourable circumstances, to be visible on our globe, where, with a good telescope, it assumes the appearance of a faint ash-coloured light, measures 2526 toises, or 15156 Paris feet.

As we are not acquainted with the laws of the inflection of light at the atmosphere of Venus, and are as much in the dark, concerning that as well as our own twilight, whether it be produced by a simple or compound reflection, this computation, it must be owned, is somewhat conjectural, and at best, conditional; and yet it supplies us with some farther means of estimating the height and density of the atmosphere of Venus with a somewhat greater precision.

If, according to the above principles and computations, it be determined that the twilight of Venus extends, at least in a considerable proportion, as far as ours; and if the inferior part

of the atmosphere, as far as a perpendicular height of 15000 feet, be so dense as to reflect so clear a light upon a zone extending 67 geogr. miles into the dark hemisphere, that we, at a distance of only 12° 47′ 45″ from the sun, and when this luminary is only from 3 to 4 degrees below the horizon, can see it so distinctly and evidently as to be able to compare it to our common twilight, measuring 6° 28′½, it must follow, that the atmosphere of Venus rises like ours, far above the highest mountains: and although we ascribe to that atmosphere the greatest possible transparency, it will still remain a more opake covering than, according to my Selenotopographical Observations, that of the moon appears to be. These observations also point out the probable cause why we do not see on this, our nearest, primary planet, even in the most favourable circumstances, any of the superficial shades, which, according to all analogy, we have reason to suppose on this, as well as on all the other planetary bodies; and which, considering the proximity of the planet, we should, when at its mean distance, and with a good HERSCHELLEAN telescope, magnifying 60 or 70 times, certainly distinguish nearly as plainly as I have seen, even with the naked eye, the superficial shades on the moon, not only the greatest ones, but also some of the lesser, such as Copernicus, Aristarchus, &c.; and surely, as well as we can discern, with a proper luminous magnifying power, the variable faint atmospheric stripes on Saturn and Jupiter, and the atmospheric opacities on Mars. At least it appears to me, that the density of the lower part of Venus's atmosphere, which enables it to reflect so clear a light into the dark hemisphere, sufficiently warrants such a conjecture: and it may well be conceived, that with so dense an atmosphere, the very

strong light of Venus must occasion a much greater degree of glare, than the soft light of the moon in a much more rarified atmosphere. In general it appears to me, that whoever will bestow some attention on these observations, and compare them impartially with the many others I have made on the atmospheres of the moon, Jupiter, Saturn, and even the sun, will find a striking coincidence between them.

The simple fact, that Venus in its different phases, and especially at the times of its greatest elongations, shews a much greater diminution of light at its terminating border than the moon, is, no doubt, sufficient to point out a much denser atmosphere than that of this satellite. And this is, moreover, fully evinced by the observations on its twilight. The circumstance also, that there are seen on this planet none of the flat spherical forms as are conspicuous on Jupiter and Saturn, none of the stripes or longitudinal spots parallel to the equator, which are seen on these planets and the sun, and which point out a certain stretch of atmosphere, gives room to infer, that the globe of Venus, with reference to its diameter, and other circumstances of physical arrangement in its construction,* performs its rotation round its axis in a much longer space of time than those planets, or the nearly similar ones of our earth and Mars; and this is actually confirmed by my observations on the diurnal period of Venus.

Thus we see a remarkable coincidence in every respect; and yet, though we cannot suppose a smaller, but rather a greater force of gravity on the surface of Venus than on our globe,

* These references have already been noticed by Chevalier DE GOIMPY, in the *Journal des Savans* for January, 1769; and before him, by M. DE MAIRAN, in the *Mémoires Acad.* for 1729. See DE LA LANDE's Astron. §. 3219, and 3122.

nature seems, however, to have raised on the former such great inequalities, and mountains of such enormous height, as to exceed 4.5, and even 6 times the perpendicular elevation of Cimboraco, the highest of our mountains. Thus are we, by these observations, led to a farther contemplation of the immense, and yet analogical variety with which the great Author of nature has dignified his works, as well in the greater objects, as in the smallest microscopic atoms; and the incessant novelty of combinations with which he has adorned them.

On the Atmosphere of the Moon.

Referring to my Selenotopographic Fragments for the proofs I there adduced of the real existence of a lunar atmosphere, which had been so frequently doubted; I shall also appeal to the same work for the observations I formerly made on several of its relative properties, compared with the same in our atmosphere, such as its greater dryness, rarity, and clearness, which, however, do not prevent its refracting the solar rays, having pointed out the circumstance, that the mountains in the dark hemisphere of the moon, near its luminous border, which are of sufficient height to receive the light of the sun, are the more feebly illuminated the more distant they are from that border: from which proofs of a refracting atmosphere, I also deduced the probability of the existence of a faint twilight, which, however, my long series of observations had not yet fully evinced.

As one fortunate discovery often leads to another, I had no sooner succeeded in my observations on the twilight of Venus, than I directed my attention to that of the moon, and applied

the calculations and inferences I there made, to some appearances I had already noticed on this satellite. It occurred to me, that if in fact there were a twilight on the moon, as there is on Venus and our earth, it could not, considering the greater rarity of its atmosphere, be so considerable: and that the vestiges of it, allowing for the brightness of the luminous part of the moon, the strong light that is thence thrown upon the field of the telescope, and in some measure the reflected light of our earth, could only be traced on the limb, particularly at the cusps; and even this only at the time when our own twilight is not strong, but the air very clear, and when the moon, in one of its least phases, is in a high altitude, either in the spring, following the sun two days after a new moon, or in the autumn, preceding the sun in the morning, with the same aspect: in a word, that the projection of this twilight will be the greater and more perceptible the more falcated the phase, and the higher the moon above the horizon, and out of our own twilight. This struck me the more, as I recollected having, two years ago, perceived such an appearance at the outward edge, near the points of the cusps, though I did not then reflect on the cause of it.

As all the requisite circumstances, however, do not often coincide, I thought myself particularly fortunate when, on the 24th of February, I was favoured with a lucky combination of them. Although this be as yet only a single observation of the sort, it is, however, in every respect so complete, and the inferences it leads to are, to me at least, so new and interesting, that I cannot withhold it from those liberal minded men, who are zealous in the pursuit of genuine philosophical knowledge.

On the abovementioned evening, at 5^h 40′, two days and twelve hours after the new moon, when in consequence of the libration, the western border of the grey surface of the Mare Crisium was 1′ 20″ distant from the western limb of the moon, the air being perfectly clear, I prepared my seven-feet reflector, magnifying seventy-four times, in order to observe the first clearing-up of the dark hemisphere, which was illuminated only by the light of our earth, and more especially to ascertain whether in fact this hemisphere, which, as is well known, is always somewhat more luminous at the limb than in the middle, would emerge out of our twilight at many parts at once, or first only at the two cusps. Both these points appeared now, most distinctly and decidedly, tapering in a very sharp, faint, scarce any where interrupted, prolongation; each of them exhibiting, with the greatest precision, its farthest extremity faintly illuminated by the solar rays, before any part of the dark hemisphere could be distinguished. But this dark hemisphere began soon after to clear up at once at its border, though immediately only at the cusps, where, but more particularly at their points, this border displayed, on both at the same time, a luminous margin, above a minute in breadth, of a very pale grey light, which, compared with that of the farthest extremities of the cusps themselves, was of a very different colour, and relatively as faint as the twilight I discovered on the dark hemisphere of Venus, and that of our own earth, when compared with the light immediately derived from the sun. This light, however, faded away so gradually towards the east, as to render the border on that side perfectly undefined, the termination losing itself imperceptibly in the colour of the sky.

I examined this light with all possible care, and found it of the same extent at both points, and fading away at both in the same gradual proportion. But I also, with the same caution, explored whether I could distinguish any part of the limb of the moon farther towards the east; since if this crepuscular light had been the effect of the light reflected from our globe, it would undoubtedly have appeared more sensibly at the parts most remote from the glare of the illuminated hemisphere. But, with the greatest exertion of my visual powers, I could not discover any part of the, as yet, wholly darkened hemisphere, except one single speck, being the summit of the mountainous ridge Leibnitz, which was then strongly illuminated by the solar light: and indeed eight minutes elapsed before the remainder of the limb became visible; when not only separate parts of it, but the whole displayed itself at once.

This alone gave me certain hopes of an ample recompence, and satisfied me that the principles I had laid down in my Selenotop. Fragm. § 525, *seqq.* concerning the atmospheres of the planets, and especially of the moon, are founded on truth. But a similar observation made on the 6th, after seven o'clock, afforded me several collateral circumstances, which strongly corroborate what I have there advanced on this subject. The whole limb of the dark hemisphere illuminated only by the reflected light of our globe, appeared now so clear and distinct, that I could very readily discern not only the large, but also the smaller spots, and among these Plato, Aristarchus, Menelaus, Manilius, Copernicus, &c. and even the small speck to the north-west of Aristarchus, marked *b*, Tab. XXVII. fig. 1. of the Fragments. I could apply the usual

power magnifying 161 times; and had full leisure, and the means to examine every thing carefully and repeatedly, and to take very accurate measurements.

Although a just idea of so delicate a phænomenon as this crepuscular light cannot possibly be conveyed by a drawing, but must be gathered from actual inspection, I have nevertheless attempted a delineation of it, and of the southern and eastern cusps fig. 1 and 2, Tab. VIII. as deduced from my measurements, especially at the southern cusp, in hopes thereby to render what I have farther to say concerning this observation the more intelligible.

The southern cusp, fig. 1. extended from *a* to *c*, with a gradually fading but still resplendent solar light, of its usual pale yellow colour, and terminated at *c* with a mountain. That this was really the point of the cusp, appears not only from the general construction of the falcated segment, which was sufficiently narrow even at its beginning *a*, near which it was somewhat disfigured at *b* by a high mountain; but also from the narrowness of its luminous curve at *d e* and *f*, the breadth of which seldom exceeded 1″, and had a sensible interruption so near as *d*. This curve was throughout, from *a* to *c*, except where the glare of the solar rays spread some degree of light, bordered with the pale ash-colour of the dark hemisphere, glimmering with the faint light reflected from our earth, out of which, however, rose the higher mountains *g*, *b*, *i*, *c*, which were now already illuminated by the sun; and farther on, not less than thirty lines, or, according to my usual projection, two minutes, distant from the point *c*, was seen another mountain *l*, which belonged to the high ridge Leibnitz, and also received its light immediately from the sun.

There can hence be no doubt of the termination of the cusp being at *c*: and this being well ascertained, I now distinguished with the greatest certainty the twilight extending from *c* to *k*. The most remarkable circumstances attending this light were, that it was broadest and brightest at *c*, and that it dwindled away and contracted towards *k*, where it lost itself in the faint glimmering of our terrestrial light; and that at the northern cusp, fig. 2, at which there do not appear to be so many mountains and inequalities as at the southern, this light exhibited the same pyramidal form, and was of equal length, and alike fading in intensity and colour, as that at the southern.

This light, compared with that of the thinnest and least bright part of the cusp *d c*, was as faint as the pale ash-coloured spots in the luminous hemisphere, when opposed to the bright ones. But this is still better illustrated by a comparison between the high mountain *l*, fig. 1, which now already appeared illuminated by the solar light, and the spot Aristarchus, which shone moderately merely with the light reflected from our globe. The said mountain had, comparatively with the thin luminous arc *d e f* of the bright hemisphere, and the mountains *g*, *b*, *i*, *c*, a very pale, fading, but yet brighter light than Aristarchus, as indeed might have been expected from what I said in my Selenotop. Fragm.; but this reflected light upon Aristarchus was however sensibly brighter than the glimmering light from *c* to *k*. And respecting the still fainter terrestrial light which bordered the luminous curve from *c* to *k*, fig. 1 and 2, I cannot give a better idea of it than by observing, that the light at the extremities of both the cusps appeared of a pyramidal form, similar to, but

though gradually fading, and very undefined, yet brighter than that of our zodiacal light, when, in the months of March and April, it blends itself, comparatively with the remaining colour of the sky, with the terrestrial light, terminating in a very sharp point.

The undefined and gradually fading appearance of this light was the cause that, though I had recourse to a dark projection table, I could not, however, take any accurate measurements of it. I found, nevertheless, by repeated comparisons, that the length of this pyramidal glimmering light, in which I could perceive no sensible inequality at the limb of the moon, amounted to about $\frac{4}{5}$ of the distance between the two mountains *c*, *l*, fig. 1, which shone with the solar light. Comparing also this southern twilight with the northern, it appeared of the same length; and on measuring the distance *c l*, I found it repeatedly = 30 lines = 2′; so that the length of the twilight must have amounted to 20 lines = 1′ 20″.

Its greatest breadth at *c* could, on the other hand, because of the extent and greater density of its light, be easily ascertained by means of the immediate application of the projection table. This measurement gave at most $\frac{4}{5}$ of a line, or full 2″.

Although I be positively certain of this very remarkable appearance at both cusps, and of its perfect similarity, in all my observations, I could not, however, trace any vestige of a like crepuscular light at any other part of the terminating border: nor could I on the very next evening, being the 25th, and also on the 26th of February, perceive, even at the cusps, any of the twilight I expected to see there; the very thin, faint, luminous line which did indeed appear on the 26th, at the southern cusp between *a* and *b*, fig. 3, being undoubtedly the

effect of the immediate solar light, probably illuminating some prominent, flat area, as yet situated in the dark hemisphere.

Thus far the observations: and now for the application of them.

I need hardly insist upon the proofs, that the very faint pyramidal glimmering light, observed on the 24th of February, at the extremities of both cusps, could by no means be the immediate effect of the solar light, all the circumstances of the observations militating uniformly and decidedly against this supposition, which, were it true, would oblige us to admit a most unaccountable diminution of light, and thence also a density of the lunar atmosphere, that ought to exceed even the density of ours; a fact absolutely contradicted by all the lunar observations hitherto made. This light, indeed, was so very faint, that it disappeared at $7^h\ 20'$, when the moon approached the horizon; whilst, on the other hand, Aristarchus, which had no light but what it received from the earth, was still very distinguishable; and the summit of Leibnitz *l*, fig. 1, (which, though far within the dark hemisphere, was, however, illuminated by the immediate solar rays) displayed a degree of brightness which, although when compared with that of the cusp *d e f*, it appeared very faint and dwindling, equalled, however, that of our Pic of Teneriffe. Nor can it be conceived why this glimmering light broke off so suddenly at both the cusps, without a progressive diminution. It can hardly be supposed that similar, grey, prominent, flat areas, of the same form and dimensions, and comparatively of a faint light, which, whilst in the dark hemisphere, they derive immediately from the sun, exist on all parts of the moon; more especially as at the places observed, the limb happened to exhibit throughout

an exact spherical form, without the least sensible inequality; and as in both the bordering regions of the northern and southern hemispheres, especially in the latter, no such grey, prominent planes are any where discernible. It may then be asked, why did this faint glimmering light appear at both cusps, along equal arcs of the limb, of equal length and breadth, and of the same pyramidal form? and why did its farther extremity blend itself with the terrestrial light of the dark hemisphere, which, according to a great number of my selenotopographic observations, is by no means the case, even with those grey prominent areas which, being at some distance on the dark side of the terminating border, are nevertheless illuminated immediately by the sun?

These, therefore, could certainly not derive their light immediately from the sun; whence this appearance, like the similar ones on the planet Venus, can only be ascribed to the solar rays reflected by the atmosphere of the moon upon those planes, producing on them a very faint, gradually diminishing, glimmering light, which at last loses itself in the reflected terrestrial light, in the same manner as our twilight blends itself with the light of the moon. Every circumstance of the above observation seems to me to confirm this supposition; and hence the observation itself, which, though single, was however a most fortunate and complete one, must appear of no small degree of importance, since it not only confirms the observations and inferences on the long contested lunar atmosphere contained in my Selenotop. Fragm. but also furnishes us with many more lights concerning the atmosphere of planets in general, than had been afforded us by all those observations collectively.

This, and the mathematical certainty that the phænomenon is, in fact, nothing but a real twilight in the lunar atmosphere, will be farther evinced by the following theoretical deductions, in which the same mode of computing as was used in the observations on Venus, will be adopted, with this difference, that the angle $f\,c\,g$, fig. 7, Tab. VI. = A c F, is equal to the angle D c E, or the geocentric elongation of the moon from the sun.

1. According to the above observation, we find that the twilight at both cusps measured, from c to k, fig. 1 and 2, Tab. VIII. 20 lines = 1′ 20″. The apparent diam. of the moon being at 6^h P. M. = 31′ 18″; whence the twilight must have extended over an arc of the limb, measuring 4° 53′ 23″; and, if the real diam. of the moon be assumed at 465 geogr. miles, this extent must have answered to 19¼ of these miles.

2. But we have seen that $f\,c$, fig. 7, Tab. VI. or the measured projection of the twilight, is by no means the true distance of its farther edge f, from the terminating border g. This distance, therefore, must be found by the following computation:

Long. of the moon, Feb. 24, 6^h P. M.	=	12° 7° 39′ 22″
Long. of the sun - - - - -	=	11 5 52 22
Difference of Long.	=	1 1 47 0
	=	31° 47′ 0″
Latitude of the moon	=	0 22 17
Log. cos. 31° 47′ 0″	=	L. 9,9294424
+ Log. cos. 0 22 17	=	L. 9,9999908
Log. dist. of ☽ from the ☉	=	L. 9,9294332
	=	31° 47′ 7″

Thus was the above the amount of the angle $f c g$, fig. 7; and as the sin. of the arc $f g$ = sin. $f c \times$ sin. $f c g$, then

Log. sin. $f c$ = L. 4° 53′ 23″ = L. 8,9306486
+ Log. sin. $f c g$ = L. 31 47 7 = L. 9,7215942

L. sin. $f g$ = 8,6522378
which gives $f g$ = 2° 34′ 25″

Whence I infer, that the breadth of the lunar twilight from the terminating border to where it loses itself in, and assumes the faint appearance of, the light reflected from our earth, measures, in a direction perpendicular to the aforesaid border, 2° 34′ 25″, equal to a breadth of 105 lines, or $10\frac{1}{3}$ geogr. miles.

3. Hence, admitting that the laws of the inflection of light are the same in the moon as on our earth and Venus, it will be easy to compute the perpendicular height of the inferior and more dense part of the lunar atmosphere $b d$, fig. 9, Tab. VII.; the angle c, in the right angled triangle $d c e$, = 1° 17′ 6″, and the radius $b c$ = 234 geogr. miles, or 891914 toises, being given; whence

Log. rad. = L. 891914 = L. 5,9503210
− Log. cos. 1° 17′ 6″ = L. 9,9998908

5,9504302 = 892140
− $b c$ = 891914

$d b$ = 226

So that the inferior more dense part of the moon's atmosphere, measures, in perpendicular height, not more than 226 toises, or 1356 Paris feet; that inferior part, namely,

which reflects so much of the solar light into its dark hemisphere as produces a twilight, more luminous than the light reflected upon its surface from the earth, when it is only 31° 47′ 9″ from its opposition to the moon, when, consequently, it shews the greatest part of its disk illuminated, and affords some degree of light to the nights in the moon.

A cursory survey of these computations, shews that they not only contribute to illustrate the observations, but also that they coincide so fully with the collateral circumstances, as still farther to confirm those observations.

1. I was well aware, even at the time I made the observations, that the lunar twilight must, as it actually did, appear in the form of a lengthened pyramid. And indeed the 7th fig. Tab. VI. which represents the twilight $c\,k$, $g\,f$, F b, before the conjunction, projecting from the cusps towards the west, clearly points out, that only its pyramidal part $c\,f\,k$ could be visible at T, whilst the whole remainder of it F $g\,c\,f\,b$ extended over the whole hemisphere of the moon A F D E C, which was turned away from the eye of the observer.

2. It appeared to me, on the other hand, before I had sufficiently reflected on these collateral circumstances, that it could not well be reconciled to theory, that this twilight, whose length from c to f measured 90 lines, should at $c\,k$, where its breadth was the greatest, not be broader than $\frac{1}{3}$, or at most $\frac{2}{3}$ of a line. I soon, however, removed the difficulty by the following deduction. From the above computation, it appears that the side $f\,g = k\,c$, or the distance of $f\,k$, the farther edge of the twilight from the terminator $g\,c$, was = 105 lines, or an arc of 2° 34′ 12″. But the arc $k\,c = f\,g$, or the greatest visible

breadth of the twilight did not lay, like the side fc, along the edge of the limb, but nearly at right angles to it, in a direction immediately tending towards the eye: whence its orthographic projection could by no means amount to 10,5 lines, but must, according to well known principles, be less in the proportion of the sine of 2° 34′ 12″ to the cosine = 10,5, whence, by the following computation,

Log. $k\,c = fg$ = L. 10,5 = L. 2,0211893
\+ Log. sin. 2° 34′ 12″ - - = L. 8,6516648
= L. 0,5

This greatest apparent breadth of the twilight at the cusps could not, therefore, exceed 0,5 lines, or full two seconds; and this in fact was the breadth given it by the observation. It is scarce possible, that in the determination of so nice an object, theory should coincide more accurately with observation.

3. But it appears farther from these observations, why, notwithstanding all the pains I took, I could perceive nothing of this twilight at the terminating border of the falcated phase, and why, on the succeeding evening, this light disappeared likewise at the cusps. The brightness of the phase will, even on the first days after the new moon, occasion a strong glare in the field of the telescope, and this particularly at the spherical border, terminating the light; so that the margin contiguous to this border, which is only illuminated by the light reflected from the earth, may, indeed, appear luminous, but must necessarily be very undefined. The smaller the phase, moreover, the less will be the projection of the twilight at the terminating border, so that on the third day after the new moon it cannot well amount to much more than 2″ at the cusps; and it will then be so far obscured by the stronger

light of the illuminated hemisphere as to be almost evanescent. The best illustration of this, and of the known fact, that in the broader phases the twilight will, on account of the strong contrasting light of the luminous part, vanish even at the cusps, is given by the spots Manilius and Menelaus. Both are situated near the centre of the hemisphere turned towards our earth; both have considerable diameters, the one of 24 and the other of between 14 and 16 seconds, and both appear very distinctly on the third day after the new moon, with a degree of light rather surpassing that of the twilight. These spots, however, notwithstanding their great diameters, vanish on account of the increasing glare of light, so soon as four days after the new moon, although they be then still at some distance from the terminating border.* How could, therefore, a faint light, measuring only 2″ in breadth, be visible on the third day after a new moon, at the terminating border? And how could it at the fourth day still appear distinctly at the bright cusps, where it is, indeed, of a more considerable extent, but still must, in consequence of its pyramidal form, soon dwindle away into a very narrow tapering stripe? This, moreover, was precisely the case in the many observations I made on the planet Venus.

From the above calculations it also appears, that the lower and more dense part of the lunar atmosphere, that part, namely, which has the power of reflecting this bright crepuscular light, is only 1356 Paris feet in height; and hence it will easily be explained how, according to the different librations of the moon, ridges of mountains even of a moderate height, situated at or near the terminating border, may par-

* See Selenot. Fragm. §. 531.

tially interrupt, or, at times, wholly prevent this crepuscular light, either at one or the other cusp, and sometimes at both. I cannot hence but consider the discovery I here announce as a very fortunate one, both as it appears to me decisive, and as it may induce future observers to direct their attention to this phænomenon.

Admitting the validity of this new observation, which, I think, cannot well be called in question, I proceed now to deduce from it the following inferences.

1. It confirms, to a degree of evidence, all the selenotopographic observations I have been so successful as to make, on the various and alternate changes of particular parts of the lunar atmosphere. If the inferior and more dense part of this atmosphere be, in fact, of sufficient density to reflect a twilight over a zone of the dark hemisphere 2° 34′, or 10¾ geogr. miles in breadth, which shall in intensity exceed the light reflected upon its dark hemisphere by the almost wholly illuminated disk of our earth; and if, by an incidental computation, this dense part be found to measure 1356 feet in perpendicular height, it may, according to the strictest analogy, be asserted, that the upper, and gradually more rarified strata, must, at least, reach above the highest mountains in the moon. And this will appear the more evident, if we reflect that, notwithstanding the inferior degree of gravitation on the surface of the moon, which NEWTON has estimated at somewhat less than $\frac{1}{6}$ of that on our earth, the lower part of its atmosphere is, nevertheless, of so considerable a density. This considerable density will, therefore, fully account for the diminution of light observed at the cusps, and on the high ridges Leibnitz and Doerfel, when illuminated in the dark hemisphere; as

also for the several obscurations and returning serenity, the eruptions, and other changes I have frequently observed in the lunar atmosphere. This observation also implies,

2. That the atmosphere of the moon is, notwithstanding this considerable density, much rarer than that of our earth. And this, indeed, is sufficiently confirmed by all our other lunar observations. I think I may assert, with the greatest confidence, that the clearer part of our twilight, when the sun is 4° below our horizon, and when we can conveniently read and write by the light we receive from it, surpasses considerably in intensity the light which the almost wholly illuminated disk of our earth reflects upon the dark hemisphere of the moon $2\frac{1}{2}$ days before and after the new moon. But should we even admit an equal degree of intensity, it will, however, appear from computation that our inferior atmosphere, which reflects as strong a light over 4° as that of the moon does over 2° 34′ of their respective circumferences, must be at least eight times higher than that of the moon.

3. The striking diminution of light I noticed, in my twelve years observations on Venus, likewise indicates that the atmosphere of that planet, which is in many respects similar to ours, is much denser than that of the moon; and this will be still farther corroborated, if we compare together the several measurements and computations made concerning the twilights of different planets. There is no doubt but that the faintest twilight of Venus, as seen either before or after the rising and setting of the sun across our twilight, is much brighter than that of the moon; and it appears, moreover, from computation, that the denser part of the atmosphere of Venus measures at least 15000 Paris feet in height, and spreads

its twilight 67 geogr. miles into the dark hemisphere, whilst the denser part of the lunar atmosphere, whose height does not exceed 1356 feet, produces a faint twilight not above $10\frac{1}{3}$ geogr. miles in breadth. Thus, as my successful observations on the twilight of Venus led me to the discovery of that of the moon, so did these latter reciprocally confirm the former: and thus, which ever way we contemplate the subject, must we be struck with the coincidence that prevails throughout.

4. But if the lunar atmosphere be comparatively so rare, it follows that the inflection of light produced by it cannot be very considerable; and hence does the computation of M. Du Sejour, according to which, the inflection of the solar rays which touch the moon, amounts to no more than $4''\frac{1}{2}$, receive an additional degree of authenticity.* Besides which,

5. As the true extent of the brightest lunar twilight amounts to $2^\circ\ 34'$, the obliquity of the ecliptic in the moon only to $1^\circ\ 29'$; the inclination of the orbit of the moon, on the contrary, to $5^\circ\ 15'$, and its synodic period, during which it performs a revolution round its axis is $= 29^d\ 12^h$; it follows, that its brightest twilight, to where it loses itself in the light reflected by the almost fully illuminated disk of our earth, must, at least at its nodes, last $5^h\ 3'$, and that it will be still longer at other parts of the orbit, according to the situation of the nodes.

6. And lastly, it being a well known fact† that the fixed stars, as they approach the moon, diminish in splendour at the most only a very few seconds before their occultations, it was

* See De la Lande's Astron. §. 1992—1994.

† See Selenot. Fragm. §. 531, with its note.

natural for me, after the successful observations I had made on the twilight of the moon, to pay particular attention to this circumstance. On the 25th February, at 6^h P. M. the sky being very clear, the limb of the dark part of the moon appeared uncommonly distinct; and only a few seconds of a degree from its edge was seen a telescopic star, of about the 10th or 12th magnitude. I counted full 20″ before its occultation, and $18\frac{1}{2}$ of these, without the least perceptible diminution of light. The star, however, began now gradually to fade, and after the remaining $1''\frac{1}{2}$, during which I observed it with all possible attention, it vanished in an instant. This observation agrees perfectly with the above computations. Although it be proved that the inferior dense part of the lunar atmosphere reflects a stronger light than that which the dark hemisphere receives from an almost fully illuminated disk of our earth; and although, considering the inferiority of gravitation on the surface of the moon, there be no doubt that this dense part, together with the superior gradually more rarified regions of its atmosphere, must extend far above its highest mountains; it is yet a fact that the breadth of this observed twilight, to where it loses itself in our reflected terrestrial light, does not measure more than 2° 34′: it is, therefore, highly probable that its greatest extent, in the most favourable phases near our new moon, can never exceed the double of the above arc, or 5° 8′; and hence we can only infer a perpendicular height of an atmosphere, capable of inflecting the solar rays, which at most measures 5376 feet: nor is it very likely that, unless accidental, and hitherto unknown circumstances should occasionally condense different parts of this atmosphere, these upper strata should materially affect the distinctness of a star seen through it.

But admitting the height of the atmosphere, which may affect the brightness of a fixed star, not to be less than 5376 feet, this will amount to an arc of only 0,94″, or not quite one second; and as the moon describes an arc of 1″ in 2″ of time, it follows that in general the fading of a star, which approaches to an occultation, cannot last quite 2″ in time; that if the appulse be at a part of a limb of the moon where a ridge of mountains interferes, the gradual obscuration will last a still shorter time; and that it may, under some circumstances of this nature, be even instantaneous.

Supplement to the foregoing Observations on the Atmosphere of the Moon.

The following observation of an occultation of Jupiter by the moon, when near its full, occurred to me by mere accident, on the 7th of April, 1792; and I was the more gratified by it, as I had long wished for an opportunity to observe such a phænomenon, and had no expectation of seeing it at this time, no mention being made of it in the Ephemerides.

The sky being very serene, and Jupiter uncommonly bright, I prepared my seven-feet reflector, magnifying 74 times, in hopes that the strong light and distinctness it afforded would enable me to compare the appearances of this phænomenon with the results which I had deduced from my late observations on the height and density of the atmosphere of the moon.

Fig 1. Tab. IX. represents the situation of Jupiter's four satellites, as they appeared, most distinctly, two of them to the westward, the second about one, and the first near two of Jupiter's diameters distant from its limb; and the two others

to the eastward, the third about seven, and the fourth near eight of the same diameters, distant from the said limb.

Fig. 2. shews Jupiter, with its belts, and of a somewhat spheroidical form, as it now appeared to me, and as distinctly as I had ever seen it. The equatorial belt, from *a* to *d*, was very apparent. It consisted properly of two zones, *a b*, and *c d*, of a brownish-grey colour, with a more luminous interval *b c*, between them. At *e* and *f* were two comparatively well defined stripes, which I had noticed for many years back, but which now crossed the whole disk; and the polar regions appeared again, from *g* and *h*, more dim and grey than the bright part of the planet. But what particularly struck me, were two nebulous undefined spots, *i* and *k*, which were sensibly darker than the principal zone *d d*; and at *l*, a still more remarkable, circular, though imperfectly defined spot, somewhat brighter than the luminous interval between the zones, and perfectly similar to the remarkable luminous spot which I had observed in 1786 and 1787, on the same part of Jupiter, and which then led me to some very unexpected inferences concerning the atmosphere of that planet.*

These favourable circumstances led me to the following accurate observation, which I was certain would prove instructive to me. At $10^h\ 40'\ 50''$, I saw the spot *i* at about the middle of its parallel; and immediately after began the occultation, than which a more distinct and beautiful one was, perhaps, never seen.

* *Beytr. zu den neuesten Astron. Entd.* p. 75. and Tab. I. fig. 6.

Immersions.

The western, preceding, first satellite disappeared behind the sharp bright limb of the moon, at $10^h\ 43'\ 12.$*

N. B. There may here have been an error of two or three seconds, as the satellite became indistinct at the limb.

The second satellite disappeared, without becoming at all indistinct, exactly at $10^h\ 45'\ 19''$.

The western limb of Jupiter came in contact, most distinctly, with the eastern limb of the moon, at $10^h\ 46'\ 32'',5$.

Jupiter's eastern limb disappeared, as distinctly, at $10^h\ 48'\ 20'',5$.

N. B. This immersion took place, as represented in fig. 3, to the eastward of Aristarchus, at about the 24th degree of N. lat.

The third satellite disappeared, after having been for about one or two seconds faint and indistinct, at $10^h\ 58'\ 57'',5$.

The fourth satellite, which appeared the least of them all, became undiscernible near the limb, and vanished at about $11^h\ 2'\ 16''$.

N. B. There may have been an error of four or five seconds.

Emersions.

The two preceding first and second satellites were here likewise of use in determining precisely the emersion of both the limbs of Jupiter, from the dark hemisphere of the moon.

* True time, determined by equal altitudes, taken on the 12th and 8th of April, and 2d and 27th of March, and by the known rate of the clocks; but as no altitudes could be taken on the 7th, there may be an error of two or three seconds.

The first appearance of Jupiter's western limb was very distinct at $11^h\ 43'\ 54''$.

Emersion of the eastern limb, as distinct, at $11^h\ 45'\ 39'',5$.

N. B. This emersion took place, as represented in fig. 4, to the north-eastward of Seneca (B, Tab. VIII. of the Frag.), at about the 23d degree of N. lat.

The emersion of the next, or third satellite, was not observed.

That of the fourth was distinct, at $11^h\ 59'\ 1''$.

This observation gave me the more satisfaction, as it singularly contributed to confirm the discovery I had been so fortunate as to make of the twilight in the moon, and the height and density of the lower stratum of its atmosphere.

Experience has sufficiently proved, that a stronger will ever obscure a fainter light; and it follows hence, that the light of a bright star approaching the moon, when full, or nearly so, will lose something of its lustre: but little can be inferred in favour of an atmosphere either of the moon, or of Mars, from the observation of CASSINI, in which, as Dr. HERSCHEL has illustrated by some observations of his own,* a star in Aquarius, of the sixth magnitude, and as yet six minutes distant from Mars, diminished in light when both were seen in the same field of the telescope. A mere apparent diminution of light, occasioned by the glare of a larger luminous object, when seen at the same time with a smaller one, in the field of the telescope, is one thing; and another thing is a real indistinctness of the small luminous body, which increases in proportion as they approach nearer to each other.

* Phil. Trans. for the Year 1784, Part II. p. 271.

It was very natural for Jupiter to diminish in brightness, when it approached so near to the moon, then almost at its full, as to be seen at the same time in the field of the telescope, which was, in fact, the circumstance of this observation; but I could not observe any progressive variation of light in the eastern and western, equally luminous, disks, proportional to their distances from the limb of the moon, much less a real indistinctness; and this neither when the limbs of the two planets were nearly in contact, nor when Jupiter was partly, or about one half, covered by the moon.

It was a sight truly gratifying to an eye accustomed to the light of the moon, or in general to similar observations, to behold how Jupiter, at its immersion, as well as emersion, being half, or more than half, covered by the moon, exhibited its belts and other parts, as distinctly, close to the limb of the moon, as it does at some distance from it: and had I not already succeeded in my numerous observations on the atmosphere of the moon, and very recently in those which enabled me to determine its twilight, I should perhaps have adopted the doubts the ancient astronomers entertained concerning the existence of a lunar atmosphere; and this the rather, as when Jupiter, in its immersion, was so far covered, that the luminous spot *l*, fig. 2, was close to the moon, I could plainly distinguish this spot, although it be in itself by no means very perceptible.

Such, however, must have been the appearances, according to my new observations and measurements of the twilight of the moon: for if it be proved that the extent of this twilight, to where it loses itself in the light reflected from the almost wholly illuminated disk of our earth, amounts to no more

than an arc of 2° 34′ of the circumference of the moon, and if it be hence demonstrable that its greatest dilatation does barely amount to 5° 8′, and the perpendicular height of that part of the lower more condensed stratum of its atmosphere, which is capable of reflecting the solar rays, and of producing some other, perhaps more remarkable, obscurities in the stars seen through it, does not exceed 5000 Paris feet, and hence cannot reach above one second of a degree above the limb of the moon; we need not wonder that so small a magnitude, which loses itself in the inequalities of the limb, many parts of which are known to be considerably mountainous, should not become sensible, especially at the approach of a body of so large a diameter as Jupiter, and when so small a magnifying power is applied. And thus may I, with confidence, assert a perfect coincidence between this and my many other observations.

The appearance, fig. 5, when Jupiter, at the emersion, the objects being particularly sharp and distinct, came forth from behind the moon, which now covered no more than one-quarter of its diameter, was truly splendid and satisfactory; and I must here particularly mention the circumstance, that the part of the moon's dark hemisphere, between its bright terminating edge *m n*, and its outward limb, bordering upon the emerging planet, *o p*, was particularly opake, and hence produced a very striking effect.

I omit entering here upon any farther considerations, and shall conclude with observing that, after the occultation was completely ended, the luminous spot *l* had at 12^h 1′ so far advanced in its parallel *d e*, as to have reached to within $\frac{1}{5}$, or at most $\frac{1}{3}$, of its whole length, of the western limb. And that

Fig
b
a
c
g. a
Fig

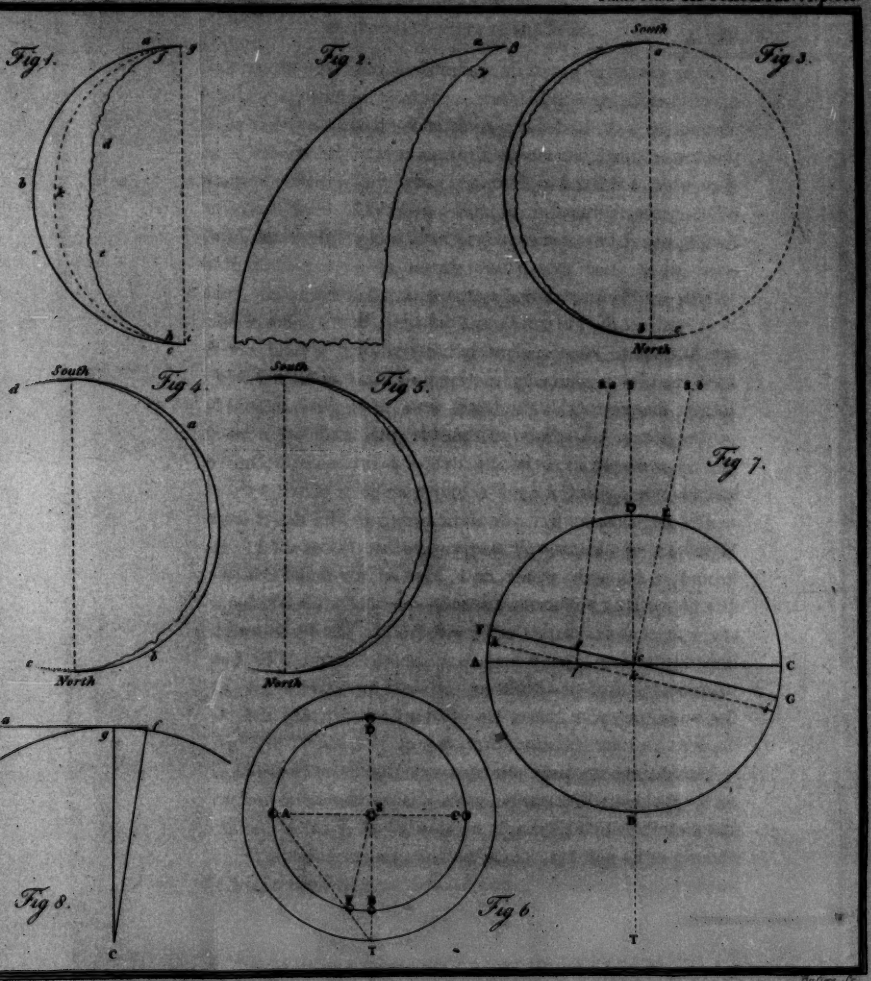
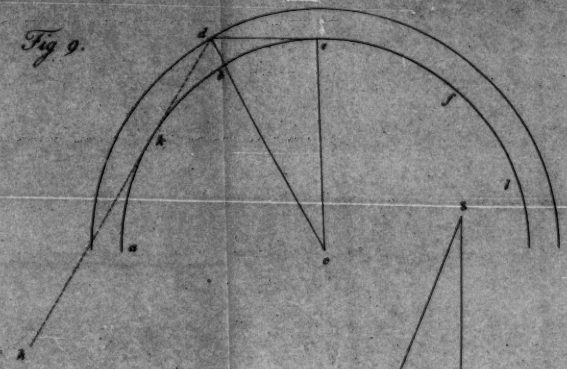


Fig 1.
Fig 2.
Fig 3.
South
North
Fig 4.
Fig 5.
Fig 7.
Fig 8.
Fig 6.

Fig. 9.



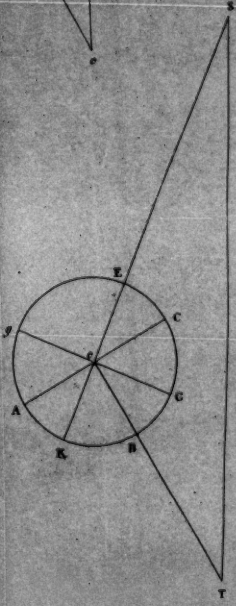
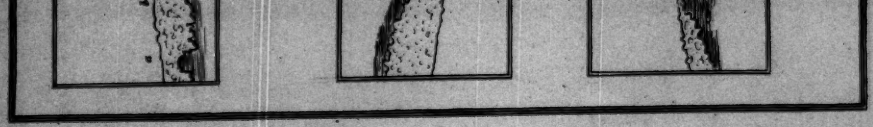


Fig. 10.



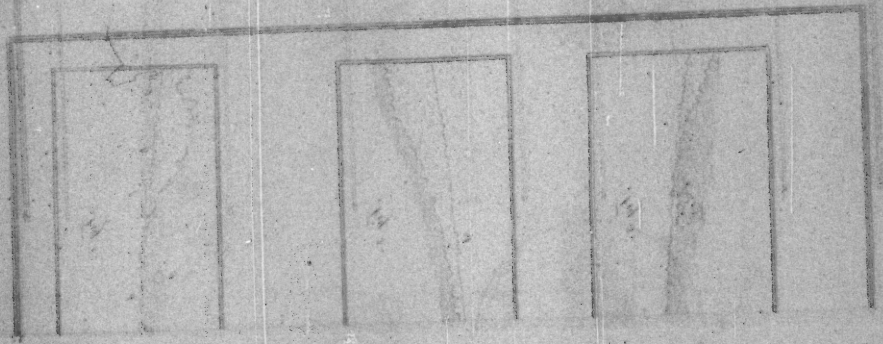


Fig 1.

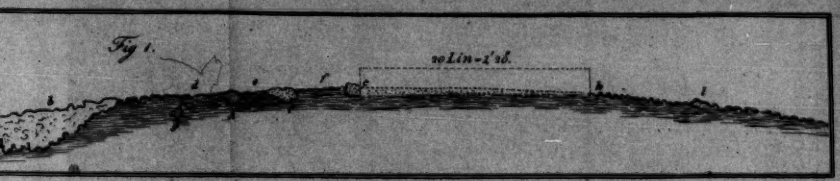


Fig 2.

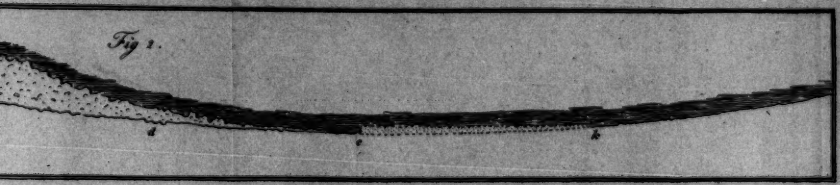
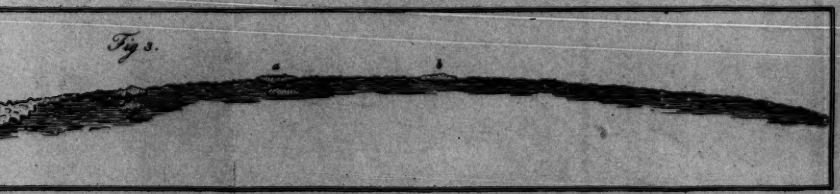
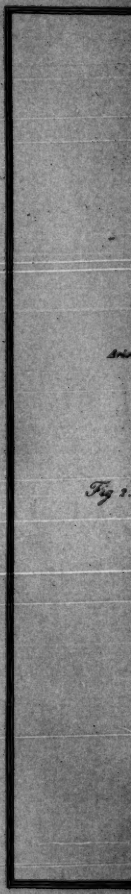


Fig 3.





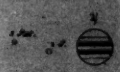
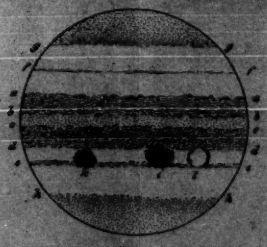


Fig 1.

Fig 3.

Fig 4.

Fig 2.



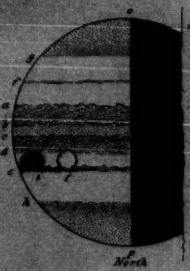
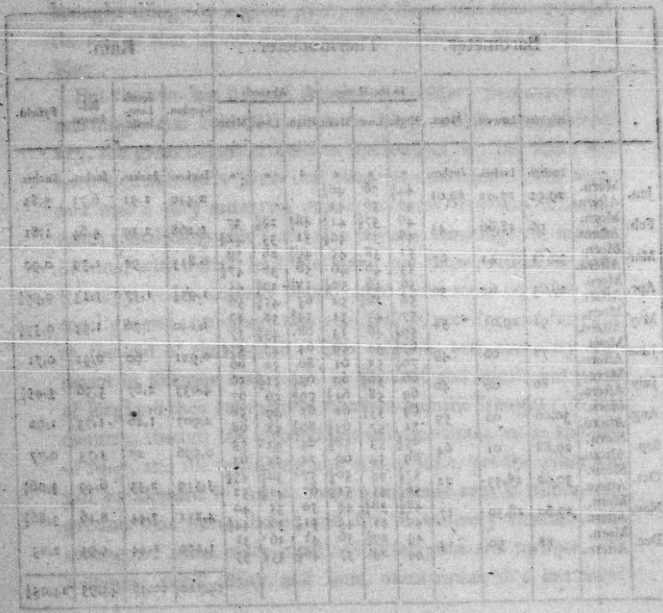


Fig 5.

on the 28th of March, five days after a new moon, I observed an occultation of a very distinct, though telescopic star, by the dark hemisphere of the moon, in which, agreeably to the above observation, not the least gradual diminution of light, or indistinctness could be perceived, the star being seen to vanish on a sudden.

Lilienthal,
April 10, 1792.



XVII. *Abstract of a Register of the Barometer, Thermometer, and Rain, at* Lyndon, *in* Rutland. *By* Thomas Barker, *Esq.*; *with the Rain in* Surrey *and* Hampshire, *for the Year* 1791. *Communicated by* Thomas White, *Esq. F.R.S.*

Read June 14, 1792.

		Barometer.			Thermometer.						Rain.			
					In the House.			Abroad.			Lyndon.	South Lambeth.	Selbourn.	Fyfield.
		Highest.	Lowest.	Mean.	High.	Low.	Mean.	High.	Low.	Mean.				
		Inches.	Inches.	Inches.	°	°	°	°	°	°	Inches.	Inches.	Inches.	Inches.
Jan.	Morn.	29,92	27,92	29,01	44½	36	40½				2,410	2,91	6,73	3,82
	Aftern.				46	36	41							
Feb.	Morn.	96	28,66	43	48	37½	41½	48½	28½	37	1,268	2,29	4,64	1,81
	Aftern.				48	38	42½	51	35	42½				
Mar.	Morn.	30,11	41	67	51	38	45	49½	28	39	0,813	92	1,59	0,90
	Aftern.				53	40	46	56	37	47½				
Apr.	Morn.	29,65	62	30	56	46	50½	53½	39½	45	1,934	1,57	1,13	0,99½
	Aftern.				58	46½	52	65	45½	56				
May	Morn.	91	29,01	54	57	45	51	53¾	38	47	1,140	76	1,33	0,59½
	Aftern.				59½	46	53	68	43½	57				
June	Morn.	78	06	49	67½	52	59	64	43	53½	0,921	60	0,91	0,71
	Aftern.				70½	53	61	80	52	66				
July	Morn.	80	00	40	66	56½	60	63½	51½	56	4,033	2,67	5,56	3,45½
	Aftern.				69	58	61½	79½	56	67				
Aug.	Morn.	30,06	02	59	69	55½	62	67	49	58	2,907	1,26	1,73	1,92
	Aftern.				71	57	63½	80½	58	68				
Sep.	Morn.	29,88	01	64	65½	53	58½	64	43	53	0,596	27	1,73	0,77
	Aftern.				69	54	60	72	55	63				
Oct.	Morn.	30,00	28,33	22	57½	44	50½	57	30½	43½	3,319	2,33	6,49	3,06½
	Aftern.				58½	45	51½	65	41½	52				
Nov.	Morn.	29,80	28,30	17	48½	38½	44	50	35	40	4,231	3,44	8,16	3,86½
	Aftern.				49	41	45	51½	36	44½				
Dec.	Morn.	88	50	14	44	29½	36	41	16	31	1,150	1,44	4,93	2,15
	Aftern.				44	29	37	46½	25½	35				
											24,722	20,46	44,93	24,05½

THE year began, as the last ended, with frost, but in a week's time the stormy wet weather returned, and, in some places, with thunder; but it gradually grew better, and by the middle of February there was a good deal of fine and pleasant weather, and, except a few frosty days and north winds the end of that month and beginning of March, the former part of spring was finer than usual; few NW winds, and those not so cold as they often are: chiefly dry in March; a good seed time, and a showery, warm, or almost hot, greater part of April, brought things on a great pace; and there was more grass at the end of that month than there sometimes is at the end of May.

But the two last days of April were colder; frequent frosty mornings, and cold NE winds in May, which being chiefly dry, the grass began to burn at the end of it; but there had been so much early grass that there was no want. The summer was a very uncertain one, sometimes very hot, at other times remarkably cold; and sharp frosty mornings the middle of June, with a little hail, and some said snow; killed, or near it, many tender summer plants; and there were very few natural ground cucumbers this year, but it was dry and burning at the end of the month. The weather was also very different in different countries: in the east and south-east parts of England they complained of a five-months drought; in this country, though the ground burned sometimes, as at the end of May, and the beginning and end of June, yet the grass had got so forward in April, that, with some rain at times, our ground was never bare; and fits of showery weather, and some great rains, made plenty of latter grass and turnips. At Selbourn, April, May, and June, were rather dry, but there

was so much rain at the beginning and end of the year, and at the middle of it, in July, that there was a very great quantity upon the whole. The crop of hay was small, but well got; the wheat remarkably good, and barley proved better than was expected; other sorts of grain were moderate, and, in general, harvest was well got.

The latter part of summer and the autumn were very fine and pleasant, and sometimes hot; generally dry, so as rather tending to burn, the beginning of October; but before the middle it began to be wetter, and after the middle more stormy, and grew colder, with frosty mornings, cutting off the late flowers; and it continued wet the rest of October, and all November, and stormy in November and December; which last month was a very disagreeable one, for storms, snow, and frost, and sometimes all at once; and that almost the whole month, for a snow and severe frost, which began the third, though with several thawing days, was hardly quite gone by the end of the year: there was but little of this frost and snow in the south of England.

Of milking Ewes.

We find in the Greek and Roman writers, and also by some passages in the Bible, that it was the custom, in former times, to milk ewes as well as cows; and, perhaps, it may better be done in those hotter climates than in England; yet I find it spoken of as having been practised in this island many years ago. TUSSER, who lived in Suffolk, and was a young man in EDWARD the VIth's time (see his life at the end of his work), mentions it as used in his time, and he approved of doing

so, as appears by the following quotations from his book, entitled, Five hundred Points of good Husbandry.

May's Abstract.

" Put lamb from ewe ;
" To milk a few.
" Be not too bold
" To milk and to fold.
" Five ewes allow
" To every cow."

May's Husbandry.

" At PHILIP and JACOB away with the lambs,
" That thinkest to have any milk of their dams.
" At Lammas leave milking, for fear of a thing,
" Lest *requiem æternam* in winter they sing.
" To milk and to fold them is much to require,
" Except ye have pasture to fill their desire,
" Yet many by milking, such heed they do take,
" Not hurting their bodies, much profit do make.
" Five ewes to a cow, make proof by a score,
" Shall double thy dairy, else trust me no more.
" Yet may a good huswife, that knoweth the skill,
" Have mixt or unmixt at her pleasure and will."

He says, five ewes will give as much milk as one cow ; and they used the milk sometimes mixed with cow's milk, and sometimes separate. Ewes should not be milked too freely, unless they have good pasture ; but with prudent management, a profit may be made of milking them ; and he is for doing

it from May-day till Lammas, but not longer, for fear of killing them. As he seems to have been bred a singing boy at Wallingford, the *requiem æternam* probably refers to some hymn he there sang.

But, I suppose, this method of milking ewes was not found to answer upon the whole, for it is left off in England now. I never was in any part of it where it is practised at present; though by the increase of inclosure, and sowing of seeds, we have better pasture for our ewes than TUSSER had. I think it must be very bad for the lambs to be weaned at May-day, which is often before the grass is fully come, though his lambs were earlier than ours; for it appears in his January's Husbandry, that the ewes were about lambing then, whereas ours do not come till Lady-day. We do not wean our cade-lambs till June, in the height of summer, and then they are not so good as those which go with ewes till the latter end of August.

Note. The foregoing Register contained also some account of the earthquake of February 25; but as an account of it is already given by Mr. BARKER, in Mr. TURNOR's paper on the Earthquake, it is omitted here.

XVIII. *Observations on the remarkable Failure of Haddocks, on the Coasts of* Northumberland, Durham, *and* Yorkshire. *In two Letters from the Rev.* Cooper Abbs, *to Dr.* Blagden, *Sec. R. S.*

Read June 14, 1792.

Sunderland, May 9th, 1792.

SIR,

THE great loss sustained by the counties of Northumberland, Durham, and Yorkshire, by the almost total failure of Haddocks, during the three last seasons, is a circumstance of serious consequence to these maritime counties, and perhaps not unworthy the notice and attention of the gentleman and philosopher.

As far back as the memory of the oldest man reaches, for about three months in the year (beginning about Martinmas), prodigious quantities of haddocks, in fine weather, were daily caught on the above coasts, which gave employment to great numbers of men, and afforded a cheap and very acceptable article of food to all ranks of people, high and low. Besides the consumption on and near the coasts, great quantities were constantly carried at least a hundred miles into and over the country.

In the winter, 1789, I am very credibly informed, and sincerely believe, that not a ten-thousandth part (I speak much within bounds) of the usual quantity was taken; and, I can venture to say, the quantity has been not greater, if not much less, for the two last seasons, to the great astonishment of the poor fishermen.

I have frequently conversed with the most experienced men in this line of business, to discover, if possible, the cause of this extraordinary failure. One man, with more religious submission than philosophic judgment, ascribes it to the will and pleasure of the Almighty; a second, to the great quantities of ballast cast by the colliers into the sea, at or near the mouths of the rivers Tyne and Wear. But this seems a very inadequate reason; for granting this act might in some small degree affect these places for a few miles, yet it could not affect the coasts at any considerable distance, either to the north or south. This last circumstance has in some degree affected the lobsters within a few miles of the two rivers, by filling up the holes and cavities in or under the rocks, where the lobsters used formerly to lie, and retreat to in stormy weather; so that being now in a great measure deprived of their old abodes of security, they are frequently, in storms and tempests, thrown on the shore, shattered and broken in pieces. A third ascribes the failure to the great number of dogfish on the coasts; but I suppose the number of them to be nearly the same, year by year. The dogfish is very voracious, and a great enemy to the fisherman and his tackle, and therefore never spared when caught: besides, it is well known that dogfish chiefly pursue the shoals of herrings, which have left these coasts before the haddocks come on. A fourth says, the shoal of haddocks

has met with beds of copperas at the bottom of the sea, and thereby is poisoned; but why should such beds (supposing the case true) have worse effects in 1789, than at any time before?

It is an indisputable fact, that many ships, on the return from Archangel, in the latter end of 1789, saw immense quantities of haddocks (no other fish were particularized), for fifty or sixty leagues, I believe, lying dead on the surface of the sea, but could not at that time ascribe any cause for the event. I believe about that time an eruption broke out in Hecla, and perhaps it may with some degree of probability be conjectured, that volcanic matter, of noxious quality, may have burst in the sea, and occasioned the above destruction and failure ever since.

The few haddocks caught in 1789 and 1790, were remarkably large; these keep nearest the shore: the small ones lie more out to sea; so that, when fishermen were wont to catch small haddocks, they desisted, and came nearer the shore to procure the large ones. The shoal generally lay about one league from the shore, was about three miles in breadth, and in length extended near the whole coasts of the three counties, in constant succession, for about three months. The breed of haddocks seems nearly destroyed on these coasts, which is a loss of many thousands of pounds *per annum* to fishermen and others, besides the loss of a very plentiful and acceptable article of food to persons of all ranks, especially in the winter season, when the price of provisions bears hard upon the poor.

May I hazard one question: Is it probable that, in the ensuing winter, or a few succeeding ones, the fishery may recover

by the return of a shoal of haddocks? For the two last winters I have waited with anxiety, but in vain, for such an event to take place.

I am, &c.

COOPER ABBS.

Sunderland, May 27, 1792.

SIR,

Encouraged by the unexpected and favourable attention paid to my letter by Sir Joseph Banks and yourself, I have, since the receipt of your favour of the 19th, daily mounted my horse, to endeavour to gain further information on the mysterious subject. Three days ago, I was fortunate enough to hear of two persons in Northumberland who were at Archangel in 1789, and waited upon them yesterday. As they lived about two miles asunder from each other, the one at North Shields, and the other at a village in the country, I had an opportunity of hearing, and asking them questions separately. Their names are, Mr. JOHN STOKER, of the Ranger, and Mr. JOHN ARMSTRONG, of the Integrity, of North Shields, masters of ships of considerable size and value, men of sober, decent character, intelligent and respected in their line of profession, from whom I received the following account, which I have every reason to believe true. That in the latter end of July, 1789, on the light passage to Archangel, after doubling the North Cape (where they joined eight or ten sail of large ships from various ports and nations), and reducing their latitude from 69 to 68, between Fisher's Island and Sweetnose, for about thirty leagues east and south, they, to their great surprise, for the space of three days, in which they had variable winds, or light airs, fell

in with immense quantities of haddocks and coalfish, and no others whatever (the last is an ordinary fish, and perhaps its name may be local here, and unknown to you, but I confess I do not know any other name it has), lying on the surface of the ocean, and sufficient, from the view they had of them for the three days, to have loaded all the ships then in company. That they found them for the space of between twenty and thirty leagues in length, and in breadth, to the east, from three to five leagues, as the ships stood off and on; but how much farther to the east, and a few other points, they might extend, these persons cannot pretend to say, such points being out of their course for the ports they were destined to. That most of the fish were dead, though some were alive, as appeared by a slight motion of the tail, but in a very weak, feeble state, and unable to sink in the water.

In the above particulars Messrs. STOKER and ARMSTRONG perfectly agree, as to the truth of the fact. The latter, through cautious timidity, prevented his crew from taking up any of the fish; but the former took on board many, both dead and in a dying state, of which he first ate, and then suffered his men to do the same: and at Archangel gave the remainder to the customhouse officers, without any person receiving the least injury. Mr. STOKER having, previous to eating the fish, tried the usual experiment at sea, of putting silver into the fresh water wherein the fish were boiled, the silver was not at all discoloured.

Talking with Mr. STOKER, in his parlour, I asked him how many fish he could take up in that or any other given space. He answered, that in various places the fish lay so thick, that in

the compass of twelve or fifteen yards a boat load, from three to five tons, might have been taken up: that he measured several of the haddocks, from two to two feet eight inches in length, and six or seven inches deep; about twice the size of haddocks on our coasts. That he opened all the haddocks he took on board, and in every one of them, both dead and expiring, he saw the *sound* much inflated or blown up, to which he ascribes the great destruction, but without being able to give any further satisfactory reason.

Mr. STOKER went from Archangel to Onega; and when Mr. ARMSTRONG, at the former place, related the story to the merchants and inhabitants at the Exchange, they replied, that they had known and heard of similar accidents; and that the great quantity of thunder and lightning, usual near the Cape, was the reason.

To the above relation, which I verily believe to be just and true, I shall not presume to add a word of my own, but submit the whole to your consideration.

In my excursion along the coast of Northumberland, I found a fisherman careening his boat, who told me that, prior to the late failure, he had frequently, with the assistance of two men, taken and sent to Newcastle, in one day, two boat loads of haddocks, containing in each from eighty to a hundred score; but in the last season he had not, in the whole, taken more than forty or fifty haddocks. He could give no reason for the failure, but another man attributed the scarcity to the want of hard gales of wind, for some years, to blow the fish off the Dogger Bank to these coasts.

The accounts received from Messrs. STOKER and ARMSTRONG

being more authentic than what I had heard before, will be a sufficient apology, I trust, for the few slight variations in my two letters.

I verily believe I cannot trace this subject further, without able instructions from gentlemen well versed in a branch of knowledge wherein I am a novice.

I am, &c.

COOPER ABBS.

XIX. *On the Cause of the additional Weight which Metals acquire by being calcined. In a Letter from* George Fordyce, *M.D. F.R.S. to Sir* Joseph Banks, *Bart. P.R.S.*

Read June, 21, 1792.

SIR,

ALTHOUGH many chemists are at present satisfied of the nonentity of what was formerly supposed to be a body, called phlogiston, and considered as an element contained in metals when in their metallic form, yet this supposition has interwoven itself so much into chemistry in general, and has been so universally received, that it may not be superfluous to relate the following experiments. If you are of that opinion, I shall be obliged to you if you will lay them before the Royal Society.

When a man begins to make an experiment, however well digested his plan may be, he finds, when he comes to put it in execution, that he must make a great number of experiments before he can bring the fact to be proved, or disproved, fairly in issue; if I may take a phrase from law, there are first many buts and rebuts. It has often been the practice to bring all this previous matter before the public. This practice may be very proper, although it does not seem so to me, and therefore I have not troubled the Society with the difficulties and disappointments I have met with; but have brought the simple experiments forward in such manner as that they can

be easily repeated by any person who is at all versed in chemistry, and possesses those most necessary qualifications in this science, patience and accuracy. I am, Sir,

Your most obliged humble servant,

G. FORDYCE.

It has been a great desideratum among chemists to determine the cause of the additional weight which metals acquire when they are calcined. To investigate this subject, I had begun the following experiment many years ago, but various other engagements have so much interrupted me, that I have had but little time to pursue any other chemical inquiry than such as were necessary to form the catalogue of the ores and minerals in Dr. HUNTER'S museum.

There is great difficulty in choosing the metal on which inquiry should be instituted, on account of the differences of their calces. After a number of trials, I chose zinc, as that whose calces appeared to differ the least from one another; in other respects there are great objections to it likewise, but which may be got over.

I took a portion of the zinc I employed, and dissolved it in vitriolic acid, with which it made a clear solution (without any of that black matter which commonly separates during its solution when we employ zinc imported from abroad). After precipitating it by an alkali, and exposing the calx to the air, it remained of a pure white; so that it could contain no iron. This zinc was reduced to its perfect metallic form by breaking it into small particles, and melting it with black flux, taking that part of it only which was at the bottom of the crucible.

I reduced this metal to a calx, by dissolving it in vitriolic acid diluted with water, and precipitated it by *kali purum* dissolved in water.

In doing this, the acid should be diluted with four or five times its weight of water, and the zinc should be dissolved very slowly, avoiding heat as much as possible during the solution. If this precaution is not taken, a quantity of volatile vitriolic acid will be produced, and spoil the experiment.

In the precipitation the alkali is apt to re-dissolve the calx, if care be not taken to use it in solution in water, and that the solution is diluted with a large quantity of water: the proportion in which the water is in *aqua kali puri* of the London Dispensatory is a convenient solution of the alkali.

Care must likewise be taken, in the precipitation, that the solution of the kali be poured into the solution of the *zincum vitriolatum* in water by a little at a time, and that the whole be perfectly mixed together before a fresh quantity is poured in, otherwise part of the calx will be re-dissolved. It is farther necessary that the exact quantity of *kali purum* be used: if too little is used, the whole calx will not be separated; if too much, part of the calx will be re-dissolved. It is also necessary that the alkali be perfectly pure, especially free from fixed air,* as that would be transferred to the calx, and as it flies off when the kali is simply united with vitriolic acid, the accuracy of the experiment would be thus destroyed.

The weight of the calx, by which it exceeds the weight

* I use the name of fixed air, although certainly not proper, in order to avoid running into confusion by employing those which have been given to this substance, until the plurality of voices shall fix an appropriated name to it.

of the metal, shews that there is a substance added to the whole metal; or, that while some substance is driven off, another is added in greater quantity; since it is clear, from various experiments well known to this learned body, that all matter gravitates, and that all the substances found in this earth, which have been tried, gravitate equally. This additional matter must be added to the metal either from the acid, the alkali, the water used in the solution, the air lying on the surface of the materials during the time of the operation, or it must come through the vessels in which the operation is performed. To ascertain this, I made the following experiment.

I took a large quantity of vitriolic acid, purified by distillation (about two pounds, it not being material what quantity was taken exactly), I diluted it with distilled water about four or five times its weight by guess (the exact proportion being also immaterial), I applied to 1000 grains of this diluted acid a sufficient quantity for saturation of *aqua kali puri*, of the London Dispensatory, rendered pure from fixed air, as is prescribed in the process of the College; I poured in the *aqua kali puri* to the diluted acid, by a little at a time, until it was nearly saturated. I then poured in some juice of violets, which gave the whole a red colour. I continued to add *aqua kali puri*, by a little at a time, until the red colour just disappeared. I added the *aqua kali puri* to the acid, rather than the acid to the alkali, because the loss of the red colour at the point of saturation can be discerned much better than the loss of the yellow colour, which the alkali intermixes with the natural blue.

I ascertained the weight of the *aqua kali puri*, by weighing the bottle containing it before any was poured into the acid,

and after the saturation took place; the deficiency of weight afterwards being the weight of the *aqua kali puri* applied to the acid for the saturation; this was 10147 grains. I also weighed the vessel with the acid before the *aqua kali puri* was poured in, and afterwards; and found the increase of weight to be exactly the same as the weight of the *aqua kali puri* and juice of violets, so that nothing was lost during the operation.

This experiment was three times repeated, taking the point of saturation from the eye. The quantities of *aqua kali puri* employed were found to be 10147 grains, 10145 grains, 10150 grains.

I took 10148 grains, being the mean of the three experiments, and applied it to 1000 grains of the same vitriolic acid; evaporated the water to dryness, and heated it to a red heat, to drive off the whole of the water; and found 978 grains of *kali vitriolatum* remaining. By this means I ascertained the quantity of *kali vitriolatum* produced from 1000 grains of the diluted vitriolic acid, when saturated with kali.

I took 1000 grains of the diluted vitriolic acid, and put it into a vessel, of the form in the figure annexed (Tab. X.), I added zinc to it until it would dissolve no more; I caught, during the solution, the inflammable air, which weighed 9 grains, and whose specific gravity was, to atmospheric air, as somewhat less than 1 to 12. The vessel contained the whole of the acid and the zinc in the globular part marked A, the acid being introduced by a funnel.

The solution was terminated in five days; when part of the tube D being broke off, it was left to stand for four-and-twenty hours, to allow the inflammable air remaining in the

vessel to fly off, and give place to the air of the atmosphere; which happened spontaneously from the different specific gravities of the two vapours.

The vessel containing the solution of the zinc was now laid upon its side, and 10148 grains of *aqua kali puri* were introduced by a crooked funnel into the globe B, being the quantity sufficient to saturate 1000 grains of vitriolic acid, as before determined. Then the tube D was hermetically sealed, and the whole weighed. The vessel was then raised, so that the globe A was undermost; this was done very gradually, so that the *aqua kali puri* was gradually added to the solution of the zinc: when a little was poured in, the vessel was brought into an horizontal position again, and shaken a little; this was repeated until the whole of the *aqua kali puri* was poured in. The zinc was thus precipitated in the form of a calx. It was suffered to stand for forty-eight hours: no alteration of the gravity took place, therefore nothing had entered through the glass to give additional weight to the zinc in order to calcine it.

The next step was to open the tube, which was done under water, and in an atmosphere of the same heat in which it was sealed, to wit, 57° of FAHRENHEIT's thermometer. The air was neither diminished nor increased, none of the water being driven into the apparatus by the weight of the atmosphere, and none being thrown out. On heating the globe B, so as to drive out some of the air, it was found to be of the same purity, nearly, as that of the atmosphere, being tried by the application of nitrous air produced from solution of mercury.

The weight, therefore, which the calx had gained, arose neither from any substance passing through the glass, nor

ftom the super-incumbent air during the precipitation. It must, therefore, be either from the acid, the alkali, or the water.

To determine whether the acid or alkali gave the weight to the calx of the zinc, I washed out the *kali vitriolatum*, formed by the combination of the vitriolic acid and the kali, with pure water, repeatedly applied, until it came away as pure as when applied, to all sensible trials. The quantity of water used was above four pounds. I evaporated this water to dryness, and heated the mass red hot, to expel the whole of the water; it weighed seven grains more than the vitriolated tartar procured from applying the acid and alkali as above. After evaporating the water, I dissolved the mass again in 40 ounces Troy weight of pure water; a yellowish powder separated. The solution of the vitriolated tartar, cleared of this powder, was again evaporated to dryness, and the water of crystallization driven off. It now weighed $976\frac{1}{10}$ grains, which is nearly two grains less than the vitriolated tartar I obtained from the acid and alkali applied simply together, without the intervention of the zinc.

The vitriolated tartar now obtained was free from any mixture. The additional weight of the calx of the zinc did not arise, therefore, from either the acid or the alkali: it remains, therefore, that it arose from the water.

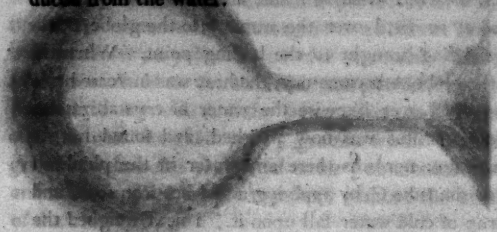
The weight of the calx of the zinc was ascertained by drying it after washing out the vitriolated tartar, heating it to a red heat, and afterwards weighing it. The weight of the zinc dissolved in saturating the acid, was 164 grains: the weight of the calx 220 grains. The additional weight was, therefore, 56 grains.

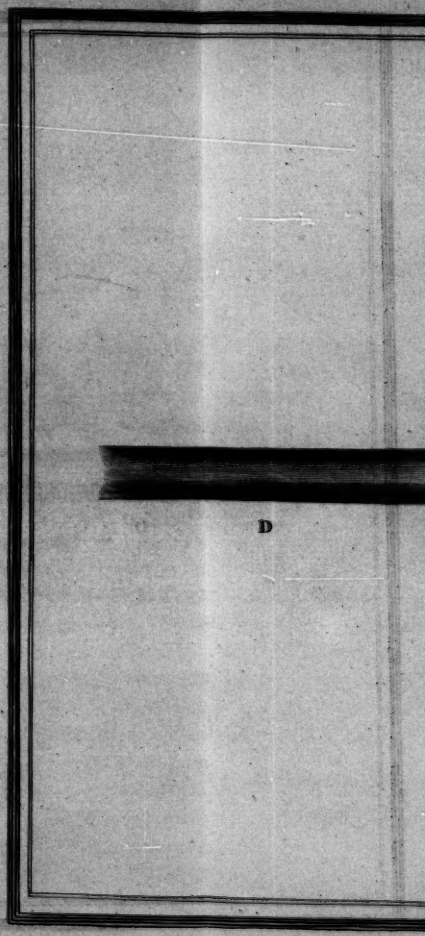
If it arose from the water, then a quantity of water, equal to the weight by which the calx exceeds the metal, must be lost in the operation. To determine this, I performed a distillation in the following manner.

I put 1000 grains of the same diluted vitriolic acid into the globe A of the same apparatus, then introduced the quantity of *aqua kali puri* found necessary to saturate it. The tube D was then bent downwards about the middle, and the apparatus brought to an horizontal position; so that the bent part of the tube was in a perpendicular direction downwards: to this I affixed a small phial, and weighed the whole. I then put the globe B in a box filled with ice, and applied heat to the globe A, so as to distil over the water into the globe B, the liquor never being brought to the boiling point. When the matter in the globe A became dry, the heat was increased to a red one, to distil over likewise the water of crystallization. The whole apparatus was now weighed, and found not to have lost a grain; nor was there any water in the phial. I then cracked the tube C, by applying a red hot iron to it, and letting a drop of cold water fall upon it. I next weighed the globe B with the water in it, then poured out the water, and let the glass dry. I weighed the glass; the deficient weight from the former weighing, being the weight of the water, was 1009⅛ grains.

I repeated the experiment, with this difference; I put 1000 grains of the same vitriolic acid into the globe A, then introduced the quantity of zinc sufficient to saturate it: I took the weight of the inflammable air as before, and found it nearly the same in weight and quality. The same quantity of *aqua kali puri* was then introduced through a funnel as in the

former experiment, then the tube was bent downwards, and a phial applied to it as before. The whole apparatus was weighed after the distillation, and found not to have lost any sensible quantity of weight, nor was there any water in the phial. The phial being detached, and the tube broken as before, the globe weighed again when dry, the deficiency was less than in the former experiment by 63 grains, which is two grains less than the additional weight of the calx above the metal and of the inflammable air taken together; and therefore the matter occasioning the additional weight of the calx above that of the metal, and the inflammable air, are both produced from the water.

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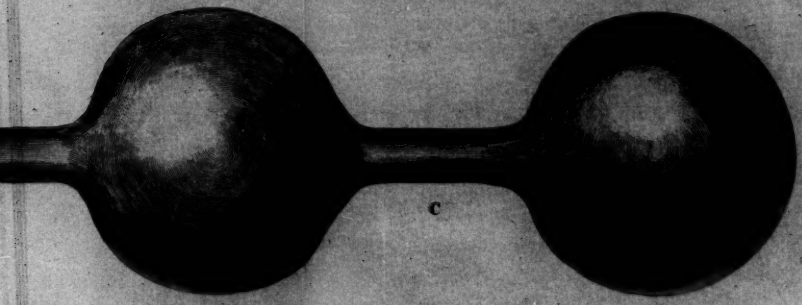
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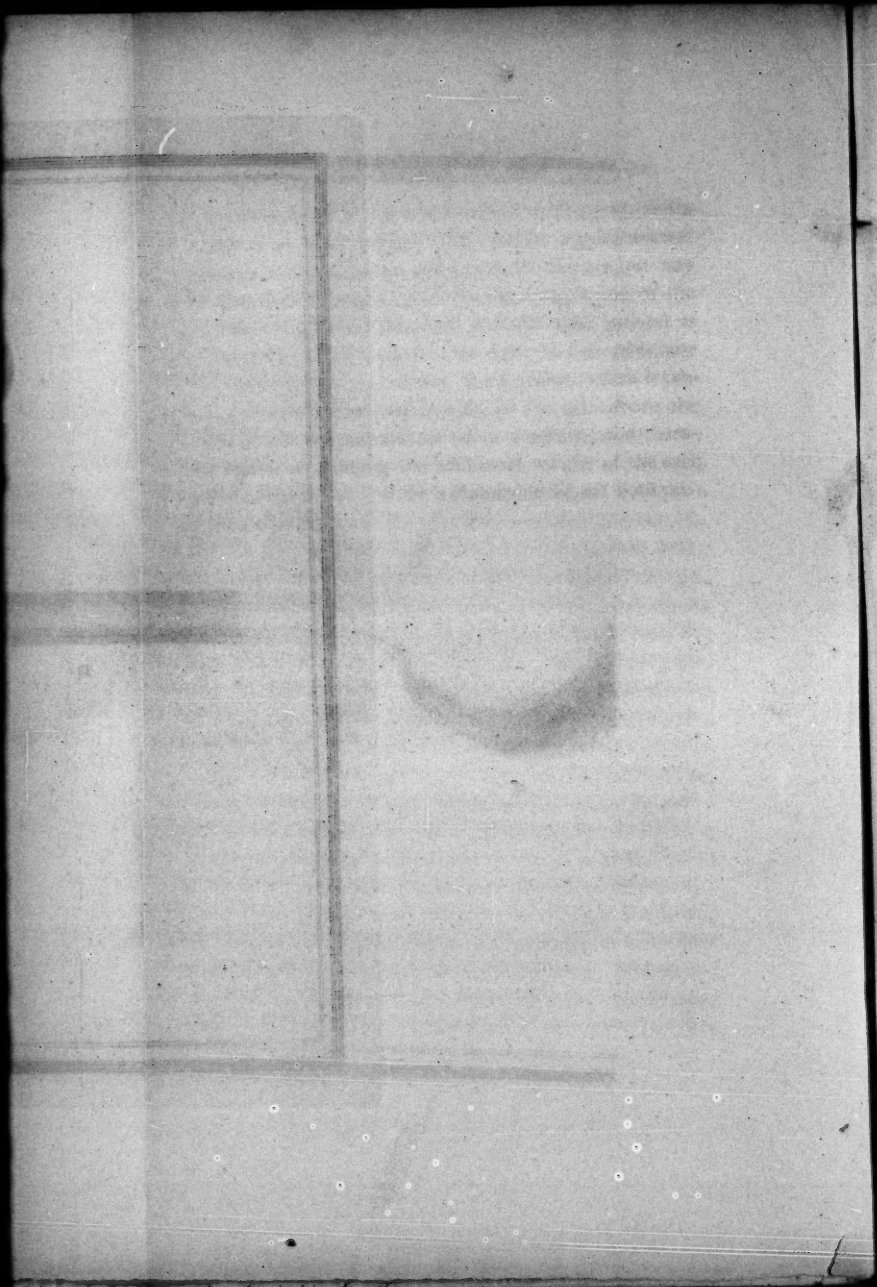

XX. *On the Civil Year of the* Hindoos, *and its Divisions; with an Account of three* Hindoo *Almanacs belonging to* Charles Wilkins, *Esq.* *By* Henry Cavendish, *Esq.*

Read June 21, 1792.

THOUGH we have received much information concerning the astronomy of the Hindoos, we know but little of their civil year, and its divisions; and what accounts of it we have received vary much from each other, owing partly, as will be seen, to different methods being used in different parts of India. As it occurred to me, that the best way by which a person in Europe could clear up the difficulties in this subject, would be to examine the *patras*, or almanacs, published by the Hindoos themselves, I applied to Mr. WILKINS, well known for his skill in the Sanskreet language, who was so good as to lend me three such, and assist me in finding out their meaning.

One of them was procured by Mr. WILKINS at Benares, and is computed for that place. The second came from Tanna, in the island of Salsette, near Bombay; but it appears to be the copy of a Benares patra, as it is disposed in the same form as the first, and is adapted to the same latitude and longitude. The third is computed for Nadeea, a town of Bengal, about 50 miles N. of Calcutta, almost as noted for learned men as Benares, and much frequented by students from the coast of

Coromandel. The language of all three of them is a corrupt Sanskreet; but the last is written in the common Bengal character.

It appears from these almanacs that the civil year is regulated very differently in different parts of India; but before I speak of this year, it will be proper to mention a few words of the astronomical, which in all parts serves to regulate the civil year.

The astronomical year begins at the instant when the sun comes to the first point of the Hindoo zodiac. In the present year, 1792, it began, according to the principles delivered in the *Surya Siddhanta*,* on April 9, at $22^h\ 14'$ after midnight of their first meridian, which is about $41'$ of time west of Calcutta; but according to Mr. GENTIL's account of the Indian astronomy, it began $8^h\ 24'$ earlier. As this year, however, is longer than ours, its commencement falls continually later in respect of the Julian year by $50'\ 26''$ in four years.

This year is divided into 12 months, each of which corresponds to the time of the sun's stay in some sign, so that they are of different lengths, and seldom begin at the beginning of a day.

The civil day, in all parts of India, begins at sun-rise, and is divided into 60 parts, called dandas, which are again divided into 60 palas.

The only parts of the Benares patras which are of any material use for my purpose, are the names of the months which are set down at the top of each page, and the three first columns, the first of which contains the day of the month, according to the civil account, the next the day of the week,

* See an account of this in the 2d volume of the Asiatic Researches.

and the third the time at which the lunar *teethee* ends; but as many may like to be informed of the nature of an Hindoo almanac, I shall give an account of the remaining parts at the end of this paper.

In those parts of India in which this almanac is used, the civil year is lunisolar, consisting of 12 lunar months, with an intercalary month inserted between them occasionally. It begins at the day after the new moon next before the beginning of the solar year.*

The lunar month is divided into thirty parts, called teethees; these are not strictly of the same length, but are equal to the time in which the moon's true motion from the sun is 12°. From the new moon till the moon arrives at 12° distance from the sun, is called the first teethee. From thence till it comes

* My reasons for saying that the civil year begins at the day after the new moon next before the beginning of the solar year, are as follow: 1st. These almanacs begin at this time, and, moreover, the year of Veekramādeetya and Sālavāhana, which is set down at the top of each page, is the same in the first page as in all the following, which would be improper, unless the year began at this time. 2dly. In the calculation of the eclipse of the sun, in PERE PATOUILLET'S Memoir, given in BAILLY'S *Astronomie Indienne*, the computation is made for the new moon preceding the beginning of the solar year, and yet the year of Sālavāhana, and of the cycle of 60, set down in the Memoir, is the same as if the solar year was already begun. 3dly. PERE DU CHAMP, in his table of the names of the years of the cycle of 60, given in the same book, has added to some of them the corresponding year of Christ, together with a day of the month. This day, in all of them, is the day next after the new moon, preceding the beginning of the solar year: and though no explanation is given, must evidently be intended for the day on which the year begins. And, 4thly. It is said in the *Ayeen Akbery*, by ABRAHAM ROGER, and, I believe, some other authors, that the year begins at this time. To the three last authorities, indeed, it may be objected, that they are taken from places in which we do not know that the Benares almanac is used; but they shew, that in some parts of India the year begins at that time, and if it does so in any place, it most likely does at Benares.

to 24°, is called the second teethee; and so on till the full moon; after which the teethees return in the same order as before.

The civil day is constantly called by the number of that teethee which expires during the course of the day.

As the teethee is sometimes longer than one day, a day sometimes occurs in which no teethee ends. When this is the case, the day is called by the same number as the following day; so that two successive days go by the same name.

It oftener happens that two teethees end on the same day, in which case the number of the first of them gives name to the day, and there is no day called by the number of the last; so that a gap is made in the order of the days.

In the latter part of the month the days are counted from the full moon, in the same manner as in the former part they are counted from the new moon; only the last day, or that on which the new moon happens, is called the 30th instead of the 15th.

It follows from what has been said, that each half of the month constantly begins on the day after that on which the new or full moon falls; only sometimes the half month begins with the second day, the first being wanting.

The manner of counting the days, as we have seen, is sufficiently intricate; but that of counting the months, is still more so.

The civil year, as was before said, begins at the day after the new moon; and moreover, in the years which have an intercalary month, this month begins at the day after the new moon; but notwithstanding this, the ordinary civil month begins at the day after the full moon. To make their method more intelligible, I will call the time from new moon to

new moon, the natural month. The civil month Visākha begins at the day after the full moon of that natural month which commences at the beginning of the civil year, or, in other words, at the day after the full moon of that natural month during which the sun enters the first Hindoo sign. Jyēshtha begins on the day after the full moon of that natural month during which the sun enters the second sign, and so on. The names of the civil months, with the names of the signs which the sun enters during the natural month at the full moon of which the civil month begins, are given in the following table, to which I have also added the day of our month when the sun entered that sign in the latter part of the year 1784, and beginning of 1785, taken from the Benares almanac, the time of the day being counted from sun-rise, and expressed in the Hindoo manner.

Civil Month.	Sign.	Day on which the ☉ enters it.
		1784. day. dan. pa.
Visākha - - -	Mesha - - - - -	April - 9, 37, 7
Jyēshtha - - -	Vreesha - - - -	May - 10, 34, 8
Āshāra - - - -	Meetoona - - -	June - 11, 0, 8
Srāvana - - -	Karkata - - - -	July - 12, 37, 58
Bhādra - - - -	Seengha - - - -	August 13, 7, 11
Aswēena - - -	Kanyā - - - - -	Sept. - 13, 7, 36
Kārteeka - - -	Toolā - - - - -	Octob. 13, 32, 55
Mārgaseersha - -	Vreescheeka - -	Nov. - 12, 25, 38
Powsha - - - - -	Dhanoo - - - -	Decem. 11, 54, 18
		1785.
Māgha - - - - -	Makara - - - -	Jan. - 10, 13, 11
Phālgoona - - -	Koombha - - - -	Feb. - 8, 40, 21
Chitra - - - - -	Meena - - - - -	March 10, 30, 38

It may be observed, that in general, Visākha begins at the day after that full moon which is nearest to the instant at which the sun enters Mesha, whether before or after; however, it is not always accurately the nearest.

The two parts of each month are distinguished in these almanacs by the addition of the syllables *vadee* and *soodha* to the name; thus the first half of Visākha, or that from the day after the full, to the day after the new moon is called Visākha-vadee, and the remainder Visākha-soodha*; but, I believe, the more usual way of distinguishing them is by the words *krēesbna paksha*, or the dark side, and *sookla paksha*, the bright side.

A consequence of this way of counting the months is, that the first half of Chitra falls in one year, and the latter half in the following year.

Whenever the sun enters no sign during a natural month, this month is intercalary, and makes an irregularity, which may best be explained by an example.

In the year 1779, the sun entered into no sign during the natural month which began at the end of the first fortnight of Srāvana; accordingly the whole of this month was intercalary, and the fortnight which preceded it was called Neeja Srāvana vadee, instead of simply Srāvana vadee, as it would otherwise have been named. The first half of the intercalary month was called Adheeka Srāvana soodha, and the latter half Adheeka Srāvana vadee, and the fortnight after the intercalary month, Neeja Srāvana soodha.†

* *Soodha* signifies clear, pure, or complete; but the word *Vadee* is not to be found in any of Mr. WILKINS's dictionaries.

† *Adheeka* signifies over and above, or intercalary. *Neeja* prefixed to the name of the month signifies that month itself.

It appears, therefore, that the two parts of the month where the intercalation takes place, are separated from each other by the interval of the whole intercalary month, and have the word Neeja prefixed to them; and the two parts of the intercalary month are called by the same name, but have the word Adheeka prefixed.*

In these almanacs no notice is taken of the solar months, notwithstanding that a column is allotted to the day of the Mahometan calendar, which seems to shew that, in the countries which use the Benares patra, it is not customary to date by the solar month; for it is very unlikely that the computers of these almanacs should have given the days of the Mahometan calendar, and yet have omitted days used in their own.

In those parts of India which use the Nadeea patra, the case is quite different. This almanac contains the name of the solar and lunar month, with the corresponding days of the week and solar month, and the number of the lunar teethee which

* What has been here said, agrees perfectly with Mr. WILKINS's almanacs; the only doubt is, whether there may not be some different method of regulating the month, which may also agree with these almanacs, and may be the true one. It is proper, therefore, that I should state my reasons for the account here given. DU CHAMP, who seems a very accurate writer, says (see BAILLY, p. 320) that he was informed by a Hindoo calculator, that whenever the sun enters no sign during a lunar month, that month is doubled. This passage agrees very well with these almanacs, if by month we mean the time between two new moons; but disagrees entirely with them if we mean by it the time between two full moons; and moreover, in Mr. WILKINS's almanac it is the period from one new moon to another, which is called Adheeka. It seems certain, therefore, that in this passage the word month must mean what I have called the natural month; and that the rule for intercalation is such as I have mentioned, namely, that it shall take place whenever the sun enters no sign during the natural month. It is certain also that the ordinary civil month begins at the day after the full moon; and granting these two points, I cannot see any way in which the months can be regulated so as to differ in substance from what I have said.

ends on those days. It begins with the day after that on which the astronomical year commences. This is marked as the first of the month, the next day is called the second, and so on, regularly to the end of the month. In like manner, all the other months begin on the day after the astronomical commencement, and the days are continued regularly to the end, so that the number of days in the month varies from 29 to 32.*

* Perhaps I do not express myself accurately in saying that the civil month begins at the day after the commencement of the astronomical. It is true, that in this almanac it is the day after the commencement of the astronomical month, which is marked by the number one; but it must be observed that the Hindoos count by years complete, not by years current: for example, the year 1000 of the Kalee Yug begins at the time when 1000 years are completed from the Kalee Yug; and it is likely that the same manner of counting is adopted with regard to days, so that the day of the month marked one, does not signify the first day, but the day which begins at the expiration of the first day, and consequently that the civil month begins at the sun-rise of the day on which the astronomical month begins. I, however, have chosen to say that it begins at the day after, partly because I am not sure that the foregoing is the true meaning of the Hindoos, and partly because it would have been difficult to express myself in such manner as not to run great risk of being misunderstood, if I had done otherwise. What is here said applies equally to the lunar month in this and the Benares almanacs.

Though it is foreign to the subject of this paper, I cannot refrain from taking notice of an error, which I apprehend many European astronomers have fallen into, from not distinguishing between days current and days complete. It is common to say that the astronomical day begins twelve hours later than the civil day, and the nautical day twelve hours sooner; and it is true that the hour, which, according to the civil account is called one in the afternoon of the first of January, is written by astronomers January $1^d\ 1^h$, but this, I apprehend, ought not to be read 1^h on the 1st of January, but 1^d and 1^h from the beginning of January, so that in reality the astronomical and nautical day both begin 12^h before the civil. A proof of the truth of this is, that in astronomical tables the place of the heavenly bodies set down for the beginning of the year, is the place for noon of the last civil day of the preceding year; and moreover, in HALLEY's tables this place is said to be *annis Julianis incuntibus*, which shews that he thought that this was not meely a practice used for the sake of convenience, but that the year actually begins at this time.

The names of the months are the same as those of the lunar months in the Benares patra, Visākha being the first, or that which corresponds with the sign Mesha.

The lunar months begin, not at the full, as in the Benares patra, but at the new moon, and are called by the name of that solar month which ends during the course of them; for example, the lunar month, during which the solar month Visākha ends, is called Chandra (or lunar) Visākha, so that each month begins a fortnight later than by the Benares patra.

The teethees do not recommence at the full moon, but are continued to the end of the month, or to the 30th. In other respects they are counted as in the Benares patra; that is, the same notation is used whenever a day occurs in which no teethee ends, or when two teethees end on the same day.

Unluckily no intercalary month occurred in the year for which this almanac was computed, so that it gives us no information about the method of intercalation; but from analogy we may conclude, that those lunar months in which the sun enters no sign are intercalary, and are called by the name of either the preceding or following month, with the addition of some word to denote that they are intercalary.*

As the Nadeea almanac begins with the day after the commencement of the solar year, and gives the day of the solar month, which the Benares patra does not, it affords reason to think that the custom of that part of India in which it is used, is to date by the solar month, and begin the year on the next

* The Chinese, who, like the Hindoos, consider that lunar month as intercalary in which the sun enters no sign, call it by the same name as the preceding month; and it is likely that the Bengalese do so too.

day to the astronomical year; and accordingly Mr. WILKINS informs me, that the Hindoos of Bengal, in all their common transactions, date according to solar time, and use what is commonly called the Bengal era, but in the correspondence of the Brahmins, dating books, and regulating feasts and fasts, they generally note the teethee; and if the year is mentioned, it is often that of Veekramādeetya, sometimes that of Sālavāhana, but more frequently the vulgar Bengal year.

From what has been said, it appears, that the Hindoo civil months, both solar and lunar, consist, neither of a determinate number of days, nor are regulated by any cycle, but depend solely on the motions of the sun and moon, so that a Hindoo has no way of knowing what day of the month it is, but by consulting his almanac; and what is more, the month ought sometimes to begin on different days, in different places, on account of the difference in latitude and longitude, not to mention the difference which may arise from errors in computation. The inconvenience with which this must be attended seemed so great to me, that two or three years ago, by the assistance of Sir JOSEPH BANKS, I proposed a query on the subject to Mr. DAVIS, author of the very valuable paper, in the Asiatic Researches, on the Hindoo astronomy, inquiring whether any method was taken to avoid the ambiguity, and was favoured with the following answer.

"My Pundit, and others with whom I have conversed on the "subject, although well aware of the circumstance (that the "month may begin on different days in different places) do "not think the ambiguity thence arising of much consequence, "nor is there any method they know of taken to avoid it.

"The almanacs in common use are computed at Benares, "Tirhut,* and Nadeea, the three principal seminaries of "Hindoo learning in the Company's provinces, whence they "are annually dispersed throughout the adjacent country. "Every Brahmin in charge of a temple, or whose duty it is "to announce the times for the observance of religious cere- "monies, is furnished with one of these almanacs; and if he "be an astronomer, he makes such corrections in it as the "difference of latitude and longitude render necessary.

"The beginning of the solar month falling on different "days of the week, is not, as I have observed, regarded; "but a disagreement in the computation of the teethee, "which sometimes also happens, occasions no small perplexity, "because by the teethees, or lunar days, are regulated most "of their religious festivals; and I am assured that an in- "stance of this kind, which occurred in Cossim Ally's time, "obliged the Rajah of Nadeea to settle by proclamation which "of the disputed computations should be regarded as the "true one."

To the best of Mr. WILKINS's knowledge, the Nadeea almanac is used all over Bengal, and the Benares all over the upper part of India: and it is likely, therefore, that the Tirhut is used all over Bahar; but of the nature of this almanac I have no information; only to judge from the date of the inscription found at Mongueer,† it is more likely to agree with the Nadeea than Benares patra.

As one of Mr. WILKINS's Benares patras came from Salsette, we may conclude that this almanac is in use in that part of

* A district in North Bahar.

† Asiatic Researches, Vol. I. p. 127.

India. The inscriptions too, found at Salsette and Dehli,* confirm the opinions that this manner of dating is in use in both those places, as both are dated by the day of the bright side of the moon.

It appears from P. DU CHAMP, and P. PATOUILLET, and I believe I may add ABRAHAM ROGER, that in the part of India from which they write, the civil year begins at the new moon before the beginning of the astronomical year;† which seems to shew that the Benares manner of dating is in use in great part of the coast of Coromandel; but there is some reason to think, that in the neighbourhood of Madras and Pondicherry, they date in a manner different from that used either at Benares or Nadeea: for Mr. GENTIL makes the month Chitra or Sitterey, as he spells it, correspond with the sign Mesh, in which he agrees with an almanac published by an European at Madras, which seems to shew that in those places they date by solar months, but make Chitra correspond with the first sign.

Mr. WILKINS thinks he has heard of one or two places on the east coast of the Peninsula, and in particular Orissa, at which almanacs are computed; but he is not acquainted with the nature of them.

I shall now give a more particular account of the three almanacs. The two Benares patras are preceded by a preface,

* Asiatic Researches, Vol. I. p. 363, and 379.

† Narsapour, from which P. PATOUILLET writes, is near the coast, and in the latitude of 16°½ N. Chrisnabouram, from which P. DU CHAMP's Memoir is sent, is in nearly the same latitude, but about 2° inland, and Paliacat, where ABRAHAM ROGER resided, is on the coast, in the latitude of 13°½, or near ½ a degree N. of Madras. This author, however, has expressed himself so inaccurately, that I am not sure whether they begin the year at that time or not.

which begins with an invocation to the Deity, and then gives a whimsical account of the four Yoogas, or ages, and of the inferiority of each succeeding age to that preceding it, and concludes with astrological remarks.

There are no titles to any of the columns of which the almanacs are composed, nor is any explanation of them given in any part of the work; but by a careful examination of the numbers, a person acquainted with astronomical computations may, without much difficulty, find out their meaning.

The calendar part contains one page for each half of the lunar month. At the top of each page is given the year of the eras of Veekramādeetya and Sālavāhana. After this comes the name of the month, and in one almanac is given also the name and number of the month used by the Mahometans.

The part below this consists of eleven columns. The first gives the day of the month, according to the civil reckoning; the next the day of the week; and the two following contain the time of the day, that is the danda and pala at which the lunar teethee ends. The fifth column contains the name of the nakshatra * which the moon quits during the course of the day; and the two next shew the time at which she quits it.

The three next columns are very odd; they serve to shew the moon's place in what may be called a moveable zodiac, the first point of which moves backwards with the same velocity with which the sun moves forwards, and coincides with the sun at the beginning and middle of the Hindoo year. This zodiac is divided into twenty-seven equal parts, and the

* Otherwise called the 27 lunar mansions.

first of these three columns gives the name of the 27th part which the moon quits during the course of the day, and the two others the time at which she quits it. I do not know what use these columns can be applied to, unless that of astrology. No trace of any thing of the kind has occurred to me in any account of the Hindoo astronomy *.

In these columns the names of the days of the week, and nakshatras, are expressed by the first syllable of the word.

The last column is the day of the month used by the Mahometans.

As no explanation of these columns is given in the almanacs, it will be proper to mention my reasons for supposing them to be such as I have asserted.

The numbers in the third and fourth column increase while the moon is near her apogee, and diminish during the rest of the month, which shews that it must be the time at which the moon completes some part of a revolution; and by examining these numbers during twelve revolutions of the moon in anomaly, it appears that the moon moves over 886 of these parts in $330^{d}\ 41^{dan.}\ 43^{pal.}$ which differs very little from the time answering to 336 teethees, so that there can be no doubt but that these columns shew the time at which the teethee ends. But a further proof of the truth of it is, that the time given in these columns for the end of the last teethee of each half month, agrees pretty nearly with the time of the new and full moon given in the nautical almanac, after allowing for the difference of longitude between Greenwich and

* From a circumstance not worth mentioning, I find that the place of the moon in this moveable zodiac, is called the Yug.

Benares, and the time between sun-rise, at the latter place, and noon; which shews also that the time in these columns is reckoned from sun-rise, as might naturally be expected.

In regard to the moon's place in the nakshatras and moveable zodiac, it appears, by examining the fifth and eighth columns, that in each of them are 27 characters, which return constantly in order, except when the regularity is broken, either by the moon quitting two spaces in the same day, or by not quitting any one space in the day. The numbers also, both in the sixth and seventh, and in the ninth and tenth columns, increase when the moon is near the apogee, and diminish when she is near the perigee, which shews that they must be the time at which the moon finishes some 27th part of a revolution of one kind or other; and by examining the alteration of the numbers during twelve revolutions of the moon in anomaly, it appears first, that the moon describes 326 of the spaces given in the fifth column, in $329^{d}\ 57^{dan.}\ 38^{pal.}$ which is the time in which the moon moves over that number of nakshatras; and secondly, that the moon describes 350 of the spaces given in the eighth column in $329^{d}\ 36^{dan.}\ 48^{pal.}$ which is the time in which the sum of the mean motions of the moon and sun are equal to 350 27ths of a circle; or in other words, is the time in which the moon's motion in the moveable zodiac is 350 of these 27th parts; and moreover, I cannot find any other 27th of a revolution of the moon which will agree with this time; which is a sufficient proof that the numbers in the ninth and tenth columns are the times at which the moon quits one of these 27th parts in the moveable zodiac. But a thing which more strongly proves the truth of this, and which also shews that the first point of this

moveable zodiac coincides with the first point of the fixed zodiac, when the sun also coincides with it, is this: according to my supposition it is evident, that whenever the sun quits a nakshatra at the same time that the moon quits some other nakshatra, the moon must at the same time quit some 27th part of the moveable zodiac; and consequently that the numbers in the ninth and tenth columns should agree with those in the sixth and seventh; and accordingly we find, that on all the days of the year, in which the sun quits a nakshatra, the numbers in these two pairs of columns are nearly alike.

Underneath these eleven columns are tables of the diurnal motion and places of the sun and five planets, and of the moon's node in the Hindoo zodiac, for each week of the year; and between these tables and the eleven columns is set down the day of the month and week, and number of the week for which these places are given, and also the interval at that time between sun-rise and midnight, and the length of the day. The day of the week for which these places are given, is that which is the first in the current solar year, and the number of the week is also counted from the beginning of the solar year. The places are given for midnight.

On the right hand of the eleven principal columns is a space allotted for miscellaneous occurrences. In this is set down the time at which the sun enters each sign, and the beginning and end of eclipses. In these two years no solar eclipses were visible, but the end of the lunar eclipse is denoted by a Sanskreet word, signifying delivery; the meaning of the term used for the beginning is not so clear. The number of digits eclipsed is not set down. The other articles in

this space consist chiefly of the time at which the moon and planets come to certain situations. Of this there is not a great deal which I understand, and what I do, is not worth taking notice of. There are also some figures and tables between the preface and calendar, which, as far as I can find, relate only to astrology.

The Nadeea almanac contains, besides the articles above-mentioned, the time of the day at which the lunar teethee ends, the number of the nakshatra and yug (place in the moveable zodiac) which the moons quits on that day, and the time at which she quits them, besides a few occasional remarks. It is disposed in a much coarser manner than the Benares patra, as each page contains as many days as it will hold, so that the month seldom begins at the beginning of a page. It contains no preface, and no explanation of the columns. The days of the week are not denoted by the first syllables of the name, but only by a number, expressing their order in the week, which caused some trouble in finding what day was meant by these numbers; but, by a variety of circumstances, I think it certain that the number 1 must denote Sunday.

XXI. *On Evaporation.* *By* John Andrew de Luc, *Esq. F.R.S.*

Read June 28, 1792.

In my last papers on hygrometry, I have considered moisture in the air as a modification of a particular fluid, produced by the evaporation of water, composed of water and fire, mixed with the air, but independent of it. However there was a more common theory of that phænomenon, in which evaporation was attributed to a dissolution of water by air: but as an inquiry into the cause of evaporation belongs more to hygrology than to hygrometry, I made then no remark on that subject; having in view some experiments which were to ascertain a particular point fundamental to it. Since that time I have made those experiments, which are the object of this paper; but before I relate them, it is necessary to explain how they connect hygrometry with hygrology; which will be by stating the principles of those two branches of experimental philosophy according to my system.

From the time I fixed my attention on evaporation, and its various consequences, I was led to think, that the kind of dissolution of water, observed in those phænomena, was operated by fire, without any interference of air: and among other reasons for that opinion, the most decisive for me was, that

every liquid cools when it evaporates; for I considered that circumstance as a proof, that the portion of the liquid which then disappears, is carried away by a quantity of fire proceeding from the liquid itself.

The general phænomena of evaporation and moisture had been my only guides, when I first published my opinion in that respect in my work *Rech. sur les Mod. de l'Atmosphère*: but it was then in a very imperfect state, and I owe much to Mr. WATT, the inventor of the new steam engine, for the degree of precision which that theory has acquired in my last work, *Idées sur la Meteorologie*. In particular, I am indebted to that truly ingenious and learned experimental philosopher, for an immediate proof of my fundamental opinion, resulting from an experiment, that he was so good as to repeat in my presence, and which demonstrates, that in the common evaporation of water in open air, the quantity of heat lost by the mass, bears to the quantity of water carried away, a proportion still greater than that which is found in the steam produced by boiling water. Therefore there is no reason to doubt, that steam is formed in the first, as in the last of those cases.

It is true, that in some other respects, appearances are very different in those two classes of phænomena; but those differences proceed from well determined causes, as will appear by the following abstract of my theory.

On the Laws of Hygrology.

1. Whenever water is in a state of evaporation, an expansible fluid, composed of water and fire, is produced. To that

fluid, the object of hygrology and hygrometry, I shall give the name of steam, in every case.

2. As long as steam exists, it has a power of pressure, as air itself; but it does not belong to the class of permanent fluids, for it may be decomposed by a certain degree of pressure, or cooling, according to determined laws.

3. The characteristic property of steam, from which proceeds its decomposition by those causes, is, the having a fixed maximum of density by a given temperature, which increases with the temperature. Thus, when that fluid is arrived at the maximum correspondent to a certain temperature, it must undergo a decomposition, either by cooling, because its actual maximum is too great for the new temperature; or by an increase of pressure (the temperature remaining the same), because its density becomes too great for that temperature.

4. The degree of pressure exercised by steam, or which it can support without decomposition, being, *cæteris paribus*, proportional to its density, depends, in the same manner, on temperature.

5. Steam is formed by every temperature, when a previous space permits its expansion; but, according to the above law, no steam can be formed when, for its formation, it ought to repel an obstacle superior, in the smallest degree, to the maximum of its power by the actual temperature; and if it has been formed under a pressure not exceeding its power, if that pressure increases, or the temperature lowers, ever so little, it is totally decomposed.

6. This is what determines both the degree of heat by which water can begin to boil, and the variations of that degree, by those of pressure. For ebullition is that state of

a liquid in which steam is continually formed within itself, notwithstanding the external pressure; and as such an expansive power in steam depends on a certain degree of density, its production requires, in the liquid, a certain degree of heat, which thus is determined by the degree of pressure. As for the fixity of the degree of heat in water boiling under a constant pressure, it proceeds from an equilibrium constantly produced between the quantity of fire which continues to penetrate the water, and that which goes off in the steam; the differences which happen in the first of those quantities, having no other sensible effect, than to produce a more or less rapid formation of steam.

7. Now from the same law derives also the difference between the phænomena of common evaporation and ebullition. If the latter requires a determined degree of heat, it is only because the steam cannot be formed within the water, without having at once a sufficient degree of density for overcoming alone the actual pressure exercised over that water by the atmosphere. But in common evaporation the steam is formed at the surface of the water by every temperature, because there it meets with no resistance but what it can always overcome: for it only mixes with the air, and expands it in proportion to its quantity, as would a new quantity of air.

8. Steam formed by common evaporation is absolutely of the same nature with that of boiling water; and in respect of the pressure that it undergoes, it is in the same state as when produced by evaporation under an exhausted receiver. In this case, where the pressure of the atmosphere is suppressed, the resistance which steam meets with in the space is its

own, and consequently it is proportional to its own power; and in open air, the part of the whole pressure incumbent on steam is, to that whole, as its power is to that of the whole mass, the rest of the pressure being supported by the air with which it is mixed: which proportion in the pressure that steam undergoes, in this case, comes exactly to that of the first. This is agreeable to the laws of expansible fluids, and the following example, taken at a determined temperature, will prove it from experience.

9. The thermometer being at about 65° of FAHRENHEIT, the maximum of evaporation, in an exhausted receiver, keeps a column of quicksilver of 0,5 inch suspended in the short manometer (I have quoted the experiments which give that middle result). That phænomenon has been considered by some natural philosophers as distinct from common evaporation, and depending on a disposition of the particles of liquids of repelling each other; which disposition, supposed to be counteracted by the pressure of the atmosphere, must appear when that pressure is removed. But, besides the known loss of heat by the liquid, in this case as well as in open air, which points out the same cause of evaporation, an equal pressure is also produced, and added to that of air, when the receiver is filled with the latter; which again indicates the same effect, as will appear in the following example.

10. If, by the above temperature, the receiver is filled with air of the same density as the air of the place; in which case a barometer inclosed in that receiver will stand at the same height as in the open air; and that in the receiver, being then very dry, water be introduced in a quantity sufficient

for producing in it the maximum of evaporation, the inclosed barometer, considered here as a manometer, will rise 0.3 inch; as Mr. DE SAUSSURE has found it by experiments.

11. If now we consider that the instrument called barometer, is in every case a manometer, we shall find, that those phænomena observed in close vessels, give us a true idea of what happens to steam in the atmosphere. When that instrument is observed in open air, it is with reason called barometer; since the expansive power exercised on its mercurial column is determined by the weight of the incumbent column of air; therefore it is the true measure of the weight of that column. But at the same time, and more immediately, it is a real manometer, since the immediate action exercised on its column, and to which this is also proportional, is, *cæteris paribus*, exercised by the density of the fluid, or fluids, which surround it, whose action is exactly the same as if any part of the mass of those fluids were suddenly secluded from the rest in a close vessel. Therefore, when steam is mixed with air, be the mass shut up in a vessel, or be it a certain part of the atmosphere distinct by its place, both fluids will act on the column of the manometer or barometer, or on every obstacle, and thus against each other, according to their respective power; for this fundamental reason, proceeding from the nature of steam as now explained, "That no me-"chanical cause can produce the decomposition of that fluid, "but by forcing its particles to come nearer each other than "the actual temperature can permit:" which case cannot happen in the atmosphere, except by the accumulation of steam itself in some part of it; since, elsewhere, it only remains mixed

with the air according to its own laws, as if there were no air.

The whole theory of hygrology, as far as I have been able to collect the phænomena of that class, appears to me comprehended in the foregoing propositions, founded on facts. The objects of that science are in general the cause of evaporation, and the modifications of the evaporated water. The common source of the water thus disseminated in the atmosphere, is the surface of the earth; whence, in spontaneous evaporation, both in air and *in vacuo*, as well as in ebullition, we see that water fly off with latent fire. If we collect that product in a close space, it acts in the same manner as a new quantity of expansive fluid. We know from experience, that an expansive fluid is really produced by ebullition, and by evaporation in an exhausted vessel: there is no reason why the cause of evaporation, and its product, should change in any case, only by the presence of air; and in examining what may happen in open air, we find no particular cause of the destruction of that expansible fluid, nor any difficulty in conceiving its dissemination in every part of the atmosphere.

But here we lose sight of steam, for it is as transparent as air itself: here also its mechanical action is as little perceivable as that of any set of scattered particles of air; and though its specific gravity is much less than that of air, its quantity existing in the atmosphere is most times so inconsiderable, that it can hardly be discovered by that means, on account of other causes which also affect the specific gravity of a given mass of free air. Therefore, notwithstanding our

experiments on the formation of steam and its effects in our vessels, we should be ignorant of its functions in the atmosphere, if it were not for its property of producing moisture, by which we may trace it wherever it is, and determine its quantity. Here then a new field is open for experiments and observations; since by connecting hygrometry with hygrology, the hygrometer is for us in the atmosphere, what the manometer is in close vessels.

The particular experiments which I have to relate, have that connection in view; as they will shew, that in a close vessel, either filled with air, or free from it, the product of evaporation affects, at the same time, the hygrometer and the manometer; the former by moisture, the latter by pression. But I must here take this as a fact, in order not to interrupt the course of the theory, which, from hygrology, leads to hygrometry, by certain laws demonstrated in the fluid which is the cause of moisture.

Of the Laws of Hygrometry.

1. The science of hygrometry derives its origin from the cause whence proceeds, that the density of steam has different maxima, according to the temperature. I have shewn, in my last work on meteorology, that in the particular association of water and fire which produces steam, both of those ingredients retain the faculty of producing their respective equilibrium between the medium and bodies; that also the particles of water retain a tendency of uniting together, which begins to be effectual at a certain distance: and that their union takes place when they come to a degree of proximity, by which

they can surmount the cause which keeps them disseminated in the space, namely, fire. The less the quantity of free fire (or of the cause of heat) in a space, the greater is the distance at which the particles of water have the faculty of uniting together, and also of following other tendencies, by abandoning their latent fire; and *vice versa*. A final union of a sensible quantity of particles of water in the medium (or, what comes to the same, that precipitation of water which first appears in it as a mist) takes place, when the density of steam is arrived beyond its limits; and those limits depend on temperature, because, the greater the quantity of free fire in the space, the nearer the particles of steam may come to one another, without a final union of their water. The whole *in vacuo*, as in air. Such is the physical principle common to hygrology and hygrometry.

2. Hygroscopic substances are of three distinct classes. Some seize on the water of steam, by a chemical affinity with that liquid; among these are acids, salts, and calces. Some only imbibe it by its tendency to propagate itself in capillary pores; but, from their nature they receive no sensible increase in their bulk by its introduction; in the number of these are porous stones. Lastly, some substances, which also only imbibe a certain quantity of water, are thereby expanded; and these are most of the solids belonging to the vegetable and animal kingdoms. Various hygroscopic phænomena, which only depend on the different properties of the substances themselves, being thus foreign to the fundamental laws of hygrometry, I shall here confine myself to the last class, which appears the only proper one for that general purpose; and, among the hygroscopes of that class, I shall only consider

those which cease to lengthen, only when they cannot be penetrated with more water; as, by the use of these, there cannot be any fallacy in respect of the most important phænomena to be determined, namely, the more or less intensity, according to certain circumstances, in the cause which affects the hygroscope placed in a medium.

3. The word moisture, like some others which also have been applied to certain phænomena the causes of which were not determined, is ambiguous in common language, implying sometimes the idea of a cause, and at other times that of an effect, without a proper determination; that defect might be corrected by means of different words for the cause and for the effect: but neologisms are often so troublesome, and sometimes so arbitrarily introduced into languages, that I shall only endeavour to determine the sense of the word moisture, according to cases, so as to avoid ambiguity.

4. Moisture, taken in a general sense, may be considered simply as invisible water, producing observable phænomena. Thus, in hygroscopic bodies, the quantity of water which expands them, and increases their weight, is concealed within their pores; and in the ambient medium, that water which affects hygroscopic bodies, being there under the form of steam, is as invisible as air itself.

5. But in respect of hygrometry, where moisture is considered as having correspondent degrees in the medium, and in hygroscopic substances, that word requires a more particular determination, on account of those two different situations of invisible water. Moisture may be either totally absent, or absolutely extreme, both in hygroscopic bodies, and in the ambient medium; which circumstance, on both sides, affords

a fixed module for determining correspondent degrees; but these modules are not of the same nature; and thence, in their relation to one another, both in the whole and in correspondent parts, moisture assumes in the medium, the character of a cause, and in hygroscopic bodies, that of an effect, as will appear by the following determination of both cases.

6. Moisture is totally absent, first in the medium, when it contains no steam; then (as a consequence) in hygroscopic bodies, because they contain no more water than can evaporate, without a decomposition of their component parts. The case supposed in that definition, is when, by some adequate cause, no sensible quantity of steam is permitted to remain in the medium; as in my lime-vessel. Moisture is extreme, first in the medium, either air or a space free from air, when no more steam could be introduced into it, without a part of it being decomposed; and then (also as a consequence to be explained) it is extreme in hygroscopic bodies, because no more water can be admitted into their pores. Now, from the nature of the last of those maxima, the quantity of water which produces it in a given body is fixed, since it is determined by the actual capacity of its pores; whereas the quantity of water which produces extreme moisture, in a medium of a given extent, is as variable as the temperature: therefore, the hygroscopic equilibrium between the medium and hygroscopic bodies in different stages of moisture, which equilibrium is the object of hygrometry as a science, does not depend on certain quantities of water contained in the first, of which the last may receive their share; it depends on different aptitudes of the steam contained in the medium to communicate water to those bodies; which aptitudes vary, not

only by the different densities of that fluid, but in steam of the same density according to the temperature.

7. In order to assign the cause of that equilibrium, we have previously to determine, 1. Whence proceeds a maximum of length in the hygroscopic bodies here referred to; 2. How that maximum is produced by steam; 3. Why it takes place by different densities of that fluid, when the temperature is different.

8. Water introduces itself into the above bodies by its property of adhering and propagating itself on their surface, and thereby of entering their capillary channels; and those bodies resist its introduction, which expands them, by the tendency of their particles to remain united. This last tendency, from its own law, being less and less in proportion as the particles of the body are already less in contact with each other, it should appear, that water ought at last to divide them entirely: but, according also to the law of that propagation of water, the greater is its quantity already induced on the internal channels of the body, the less is the tendency of new particles to enter them; and this last diminution being more rapid than the former, there comes a point in which the two forces are in equilibrium; which circumstance determines the maximum of the introduction of water, and consequently that of the lengthening of the body.

9. The maximum of length of those hygroscopic bodies is thus produced, whenever a sufficient quantity of water stands or floats round them, without having a stronger tendency elsewhere. Therefore that maximum is produced when such a body is immersed into water; and it is also produced when the same substance stands in a space where

steam is arrived at its maximum; for that fluid is then so ready to abandon its water, if this has a tendency elsewhere, that a small quantity of new steam introduced in the space, would occasion the decomposition of part of it, followed by a spontaneous precipitation of water.

10. But we have seen above, that steam arrives to its maximum with different degrees of density, according to the temperature; consequently, whatever be the actual density of steam, if that density is its maximum conformable to the temperature, the hygroscopic body will receive water to the maximum; and thereby it will attain its maximum of length.

11. Now, as according to the premises, the density of steam is not the only circumstance by which, at the last period, the hygroscopic body retains in a medium the maximum of water that it can receive, that effect depending also on the temperature; by the same cause, the density of steam is not at the former periods, the only circumstance which determines the quantity of water that the hygroscopic body can retain in the medium; that quantity depends also on the temperature. The following is the theory of that phænomenon, or of the hygroscopic equilibrium between the medium and bodies.

12. In consequence of the above determined composition of steam, whatever be its quantity in a given space, either filled with air or free from it, its aptitude of communicating water to hygroscopic substances, is always proportional to the actual ratio between that quantity and the maximum correspondent to the temperature; which ratio, and not an absolute quantity of steam, is a certain degree of moisture considered in the medium; whereas moisture considered in a

certain hygroscopic body, is the determined quantity of water which it can retain in the medium, according to the actual *ratio* in the steam. Moisture in the medium is greater, and an hygroscope retains in it a greater quantity of water, when the actual quantity of steam bears a greater proportion with its maximum correspondent to the temperature; and *vice versa*. Therefore the divisions of the instrument are meant to indicate, by every temperature, the actual distance of moisture in the medium, either from its zero, or from its maximum; and that distance is the degree of moisture. But this is yet without any reference to the density of steam; the determination of this having the temperature for one of its indispensable elements.

13. From that derives the remarkable phænomenon of the effects produced on moisture in the medium, by the variations of temperature, though the quantity of steam, or evaporated water, remains the same in the same space; effects of which we are informed by the variations of the hygroscope, and which are the same by the absence or presence of air. Every change in the temperature, operates a correspondent change in the module of moisture; or in a variable *totum*, of which, according to its magnitude, the actual quantity of steam is a greater or a smaller aliquot part; that module being always the quantity of steam that could not be exceeded in the space, by the actual temperature, without the decomposition of part of it; which is always extreme moisture in the medium.

14. From that results lastly; that if, besides being acquainted, by the hygroscope, of the degree of moisture in the medium, or of the *ratio* between the density of steam and its actual module, it is required to know the density of that

steam, or its quantity in a given space; the observation of the temperature is required: as this, by previous experiments in order to determine the quantities of steam correspondent to its different maxima according to the temperature, will afford a co-efficient for the *ratio* observed.

15. But are we permitted to consider the variations of the hygroscope as proportional to those of moisture in the medium? This, according to the above determinations, would be the case, if the hygroscopic substance of the instrument lengthened in proportion to the quantity of water that it may retain in the medium. But the cause of the expansion of those substances by water, and the capacity of their pores at different periods of moisture, are too complicated for answering that question *à priori*; and by experience, the great differences observed in the marches of many of those instruments made of different substances, prevents us from assigning that property to any of them, till some particular experiment comes to help us in that respect. However, that circumstance affects only the practical part of hygrometry, and is foreign to the fundamental principles of that science, which I have now exposed as they appear to me from its general phænomena.

I have indicated, in my last paper, two means which I had formerly imagined for obtaining that desirable and still wanting correspondence between the march of a determined hygroscope, and that of moisture in the medium. One of those means was, to observe, at the same time, the variations in weight and length of the same substance, in order to compare the quantities of water which it retains, with their effects on its length. I have executed that experiment; but its re-

sults, which I gave in my last paper, have confirmed my doubts, on even the acquisitions of weight being proportional to the increase of moisture in the medium; since they do not keep the same pace in different substances.

The other means was, to introduce in a dry vessel successive equal quantities of water without opening the vessel, and to observe their effect on the hygroscope. I made, last year, a first attempt of that experiment, which succeeded in respect of the introduction of water in a space of a known small degree of moisture; but the event confirmed also the uncertainty that I suspected in that method, because of a variable share of water retained by the vessel itself. However, as some parts of my apparatus might contribute to that uncertainty, I intend to correct it, and repeat the experiment; which, if I obtain regular results, I shall have the honour to communicate to the Royal Society.

I have now summed up the series of propositions which connect together in one system the whole of the fundamental phænomena of hygrology and hygrometry; and the only part of that system which remained to be proved by immediate experiments is, that *link* mentioned above between the two classes of phænomena, namely; "That *in vacuo*, as in air, "the product of evaporation affects the hygroscope as it "does the manometer." I had ascertained that fact since the year 1776, by immediate experiments; which was the reason why, in my last work, I considered that circumstance as a principle, which, at least, could not be denied by any thing known to the contrary. But, for an immediate demonstration of it, many conditions were wanting in my first experiment, which could not be obtained but by the whole of

my subsequent labour. That experiment is now made with a sufficient degree of regularity; and the more so, as it has been executed by Mr. HAAS, in one of his air-pumps, with some of my whale-bone hygrometers, made by himself: and I shall now give its result.

Experiments on Evaporation, in Air and in Vacuo.

The purpose of these experiments being to shew, that the effects produced by evaporation on the hygrometer and the manometer, are the same in the absence of air, as when they take place in air; I shall begin here by referring to Mr. NAIRNE's paper, in the Philosophical Transactions for 1777, in which it is proved by experiments, that Mr. SMEATON's pear-gage is not a true manometer, since it does not indicate the presence of steam, when some remains in an exhausted receiver; but that it is the only true measure of the quantity of air itself remaining in that space.

That property of the pear-gage, proceeds from the fifth law of the above theory of hygrology. When, in the common temperature of the atmosphere, steam is unprotected by a sufficient mixture of air, it is totally decomposed by the pressure of the atmosphere, when this acts by the interposition of some sort of piston, as is the column of quicksilver in the pear-gage: therefore no fluid can remain at the top of that instrument when filled with quicksilver, except the quantity of air which was contained in its capacity before the introduction of the quicksilver.

When some lasting cause of formation of steam happens to be within the air-pump, and that the pump is long worked,

the air is at last almost completely exhausted, and the remaining pressure acting on the manometer is that of steam. Mr. NAIRNE has shewn me an instance of that effect, in which the quantity of air indicated by the pear-gage, was not above $\frac{1}{10000}$ part of the whole. That remarkable fact will prove, that in the following experiments, air itself was exhausted to the utmost degree, and thus reduced to nought in respect of any influence on the main phænomena.

I must also premise, whence proceed some anomalies which will be observed in those experiments. It is in general from the nature of steam, from which, in our experiments, it is submitted to certain modifications, depending on its laws, but which cannot be always referred to them by immediate explanation, because of undetermined effects of the sides of the vessels, and of irregularities in the temperature.

The law of steam, which is the most subject to anomalies by those causes, is that of two distinct maxima, one of production, the other of preservation. Those two maxima coincide, when the temperature is near the freezing point; as in that case, the whole quantity of steam that can exist at one time in a space, is also produced when there is a sufficient quantity of water in any part of it; whereby the hygrometer stands at 100, which is its point of extreme moisture. But in proportion as the whole apparatus grows warmer, the maximum of production falls short of that of preservation; the quantity of steam does not increase, though it might be more dense without undergoing a decomposition; and thereby the hygrometer recedes from extreme moisture.

Such is the general law of steam, as it results clearly from the whole of my experiments; but in particular cases, it is

subject to anomalies from various causes, among which are the following. If the water that evaporates is warmer than the space which receives the steam, more moisture is produced in that space, or the quantity of steam is greater in it than by an equal temperature in both; and *vice versa*. More or less distance of the part of the space where the hygrometer stands from the sides of the vessel, produces also anomalies; as according to their own state of moisture, if near enough, they have an influence on moisture in that space, as I have explained in my former paper; and this is often the case in some measure when the vessels are too small. Lastly, differences in the temperature of the whole or of some part of the vessel, comparatively with that of the space, are the most common causes of anomalies; for steam is alternately decomposed and reproduced by those differences, and when they have once begun in a vessel, there is no certain means to bring it to a regular course of phænomena, except by beginning again, or by a long equal temperature.

I come now to the experiments, in which I shall indicate some effects of those causes.

FIRST EXPERIMENT.

The receiver used was about eight inches in diameter, and twelve in height. One of my hygrometers was suspended in it, with a thermometer, the degrees of which were actually divided in ten parts. In the evening, a very large wet cloth was placed in loose folds on its bottom, and the first observation was made the next morning as follows.

Time.	Long Cage.	Short Cage.	Hygrom.	Therm.
7h 5′ A. M.	0.0	———	96,5	47,75

The maximum of evaporation was then surely produced; but it remained short 3,5 degrees from extreme moisture; this was in air.

I could not have any indication of the quantity of pressure produced by the steam, mixed then with the air; for a receiver, equally pressed in and out, is not air tight. An experiment for determining that increase of pressure, must be made in a sealed vessel, as in Mr. DE SAUSSURE's experiment, by which he found that important phænomenon: but from that experiment we are sure, that if a barometer had been inclosed in the above receiver, being then sealed, the steam produced would have made it rise, at the same time that it affected the hygrometer.

The pump was then worked; after which the instruments were found as follows.

7,15	26,0	——	97,0	44,75

I shall make here a remark on the known phænomenon of the diminution of heat, when a space filled with air is suddenly exhausted; as in this case we have an analogy that will enhance the most natural explanation of that phænomenon. Free fire, the cause of heat, being an expansible fluid, as air and steam, is then carried away with them, notwithstanding its tenuity, and heat is diminished in the space till new fire is come through the vessel. Now, the same is observed in respect of moisture with a quick hygrometer, for it goes for an instant to dryness, while the quantity of steam carried away is repairing by a new evaporation.

In the above case, moisture was lastly increased 0,5 degree; yet the quantity of steam was less than before, and the cause

of that increase of moisture was, the diminution of its module by that of heat.

In respect of air, its quantity was so diminished, as to permit a column of quicksilver of 26 inches to rise in the long gage.

The pump was again worked, and time given for the instruments to settle.

7,25′	28,7	———	96,5	47,75

The original temperature was now restored; and as air had been much diminished by the first operation, fire, thus much more free in this, escaped the action of the pump. Moisture also was restored nearly to its original degree, though very little air remained in the receiver.

The pump was worked by intervals.

10,5′	28,5	0,70	95,66	47,75

The quantity of air was now so little, that it cannot be supposed to have had any share in the effect of the steam; however moisture remained sensibly the same by the same temperature.

Worked again the pump to the best vacuum.

10,10	28,7	0,53	94,5	48,8

The pressure on the short gage was now, according to Mr. NAIRNE's experiments, almost wholly produced by steam; yet moisture preserved its own distinct modifications: here it is diminished, notwithstanding the maximum of evaporation, because of the increase of heat.

The pump was worked at different times, for extracting some air that was disengaging itself from the large wet cloth.

11,35′	28,7	0,53	91,0	51
2,50 P. M.	28,72	0,60	86,75	54,15

Here again, notwithstanding a constant maximum of evaporation, moisture continued to diminish by the increase of heat, as it does in air.

The apparatus was left in that situation about four hours; and the observers having left the room, this, and the approaching evening, occasioned a sensible diminution in the heat; whereby, as the sides of the vessel cooled before the inclosed space, the steam began to be decomposed against them, and they were found covered with dew and streaming water, by a real continuing distillation. The last cause of anomaly above mentioned is here conspicuous by its great effect; for, during that distillation, and consequently though new quantities of steam ascended continually through the space, moisture was in it much less than in the former observations, as will be seen by the following, made before and after working the pump. The thermometer could not be observed, on account of the thickness of the dew on its side of the vessel.

4,20′	27,35	———	76,0	———

Worked the pump.

4,35′	27,75	0,62	76,0	———

The steam continued some time to decompose itself against the sides of the vessel, and moisture to remain in that state of diminution, till the heat communicated by the observers to the apparatus made the internal dew evaporate; and when thereby the thermometer could be observed, the instruments were as follows.

6,47′	28,18	0,6	96,0	52,75

Here also is an instance of what I have mentioned above,

of the irregularities attending all the phænomena, after some water has been once deposited on the sides of the vessel: moisture was too great for the temperature, and it remained in that state the whole evening.

7,55′	28,82	0,6	96,5	52,75
8,4	28,82	0,6	96,5	52,—

But the next morning, after the dissipation of some dew that was on the inside of the vessel, and after having worked the pump, the instruments were found as follows.

7,35′ A. M.	29,12	0,6	97,66	44,75

Now this, in respect of moisture, and its correspondence with the temperature by the maximum of evaporation, is sensibly the same, as when the receiver was filled with air at the beginning of the experiment, though at this time no sensible quantity of air remained in it.

SECOND EXPERIMENT.

Every part of the apparatus was the same in this experiment as in the former, except the receiver, which was only about six inches in diameter, and eight in height; to which circumstance may be assigned, that moisture was generally greater comparatively with the temperature; but in respect of the indifference of air to the phænomena of moisture, the results were the same.

A wet cloth, only sufficient to keep up the maximum of evaporation throughout the whole experiment, was used this time; by which means no air appeared in the receiver after the disengagement of the little quantity contained in the small cloth. This was put under the receiver, filled with

air, at about four o'clock in the afternoon: and the first observation, made in that state, was thus:

8,— P. M.	0,0	———	95,0	65,75

The pump was then worked, for extracting a part of the air, and time given for the instruments to settle, and they were found,

8,15′	6,5	———	94,0	65,75

After this observation, and after each of the following, the pump was worked, and time also given for the instruments to settle.

8,30′	13,0	———	91,0	66,75
8,45	19,0	———	86,5	67
9,15	25,0	———	86,0	67,75
9,30	29,8	0,65	85,0	67

After this, the pump was no more worked the same day, but the two following observations were still made.

10,0′	29,23	0,65	89,0	66,5
11,0	29,23	0,65	95,0	64,5

The apparatus was left in that situation till the next morning, when the following observation was made before working the pump.

6,20′ A. M.	29,35	0,55	97,5	63,15

The pump was then worked, and the best possible vacuum was produced, in which, according to Mr. NAIRNE's experiments, there could not be any sensible quantity of air.

6,40′	29,40	0,50	96,5	63,15

In comparing the results of this experiment with those of the first, moisture, as I have said, is generally greater in proportion to the temperature. But, setting this aside, and comparing the motions of the hygrometer and the thermo-

meter, it is evident, that they are independent of the modifications of air; and that it may safely be concluded: "That "the product of evaporation is always of the same nature, "namely, an expansible fluid, which, either alone or mixed "with air, affects the manometer by pressure and the hy-"grometer by moisture, without any difference arising from "the presence or absence of air; at least without any per-"ceived hitherto."

XXII. *Supplementary Report on the best Method of proportioning the Excise upon Spirituous Liquors.* By Charles Blagden, *M. D. S. R. S.*

Read June 28, 1792.

THE Report to which this paper is intended as a Supplement, was drawn up, and published, when our experiments on the specific gravities of the spirituous liquors had been continued only to equal quantities of alcohol and water by weight. It was foreseen that a further set of experiments, on more dilute liquors, would be wanted: but as these must necessarily take up a considerable time, the persons concerned thought it best to submit those already made to the public; that if any errors or inaccuracies should be discovered they might be avoided, and if any person should suggest a better method it might be adopted, in the subsequent proceedings. Want of ice, and some other hinderances, prevented the experiments on what may be called watery mixtures from being entered upon earlier than the beginning of last winter. Fresh spirit was distilled for the purpose by Mr. SCHMEISSER, who brought some of it to the specific gravity of ,817; but it had a smell somewhat different from that employed in the former experiments, and more approaching to the odour of ether. On inquiry we found, that, whereas Dr. DOLLFUSS had drawn the former spirit off vegetable alkali, Mr. SCHMEISSER used GLAUBER'S salt

calcined by exposure to the air. In order to try whether this circumstance made any difference in the quality of the new spirit, Mr. GILPIN mixed some of it with an equal weight of water, and afterwards brought the mixture to all the different temperatures from 30° to 100°, operating in the same manner as he had done with Dr. DOLLFUSS's spirit; when the specific gravities were found to come out the same. Mr. SCHMEISSER's alcohol, therefore, was used without hesitation. As no censure had yet been passed on our former experiments, the same general method was pursued for the new series; with a small variation, however, the reason of which is now to be explained.

In the Report on the first experiments I introduced the following remark. "It must be observed, that Mr. GILPIN "used the same mixture throughout all the different tem- "peratures, heating it up from 30° to 100°; hence some "small error in its strength may have been occasioned, in the "higher degrees, by more spirit evaporating than water; but "this, it is believed, must have been trifling, and greater in- "convenience would probably have resulted from interposing "a fresh mixture." The consciousness that such a source of error existed, made us desirous of ascertaining to what quantity it amounted, by some previous experiments, before the new set should be begun. These shewed that it was somewhat greater than had been supposed, though not such as ever to cause a difference of more than a single unit in the third place of decimals, even in the temperature of 100°. The greatest difference found, in that degree of heat, was ,00094; and in a heat of 80°, the highest to which the tables for use were to be carried, it amounted only to ,00064; being in both cases

greatest when the mixture consisted of 85 parts of water, by weight, to 100 of alcohol. This difference, however, small as it was, afforded sufficient reason for repeating all the former experiments, conjointly with the new set for dilute spirits, so as to make one entire series, with the same spirit, and executed throughout in an uniform manner. To obviate the error from evaporation in this series, and ascertain what each mixture really lost of its strength during the operation, all the fluids were first weighed at 60°, before they were cooled down to 30°; from 30° to 100°, they were weighed at every 5 degrees, as before, consequently a second time at 60°; and lastly, after having been heated to 100°, they were again brought to 60°, and weighed at that point a third time. The difference between these weights, at the beginning, middle, and end of the experiment, was applied, in due proportion, to correct the numbers of the respective intervals between them; by which means it is believed that the error arising from the gradual evaporation of the spirit, during the experiment, has been made to disappear. Mr. GILPIN having also observed, that the spirits adhering to the sides of the funnel which he employed to fill up the weighing-bottle, grew weak by the evaporation, and so diluted the fresh spirit poured into the funnel, determined to use a smaller instrument of this kind, namely, such an one as would not hold more than 15 grains of spirit; in which a less surface being left wet when the spirit ran out, the error from this cause would be proportionably diminished.

Under all these precautions were the experiments made, of which the results are given in the following tables. They are drawn up exactly like the tables in the former Report;

but, as alcohol was taken for the fixed quantity in the first half, so water is taken for the fixed quantity in the last half, which, therefore, consists of mixtures containing all 100 grains of water, with 95 grains, 90, 85, and so on successively, of alcohol, till the last column is pure water. This arrangement will be clear to every one, on reading the title of each column of the tables. The first table gives the actual weights, at the even degree of the thermometer, corrected for the evaporation; and the second table gives the specific gravities, calculated from those weights, with the same allowances and corrections as were specified in the original Report.

TABLE I.

[*To face p.* 428.

Weights at the different degrees of temperature.

Heat.	The pure spirit.	100 grains of spirit to 5 grains of water.	100 grains of spirit to 10 grains of water.	100 grains of spirit to 15 grains of water.	100 grains of spirit to 20 grains of water.	100 grains of spirit to 25 grains of water.	100 grains of spirit to 30 grains of water.	100 grains of spirit to 35 grains of water.	100 grains of spirit to 40 grains of water.	100 grains of spirit to 45 grains of water.	100 grains of spirit to 50 grains of water.	100 grains of spirit to 55 grains of water.	100 grains of spirit to 60 grains of water.	100 grains of spirit to 65 grains of water.
°	Grains.	Grains.	Grains.	Grains.	Grains.	Grains.	Grains.	Grains.	Grains.	Grains.	Grains.	Grains.	Grains.	Grains.
30	2487,35	2519,92	2548,42	2573,80	2596,66	2617,30	2636,23	2653,73	2669,83	2684,74	2698,51	2711,14	2722,89	2733,87
35	2480,87	2513,43	2541,84	2567,26	2590,16	2610,87	2629,92	2647,47	2663,64	2678,60	2692,43	2705,14	2716,92	2727,87
40	2474,30	2506,75	2535,41	2560,74	2583,66	2604,50	2623,56	2641,08	2657,23	2672,30	2686,38	2698,94	2710,81	2721,83
45	2467,62	2500,14	2528,75	2554,09	2577,10	2597,98	2617,03	2634,64	2650,87	2666,04	2679,99	2692,77	2704,57	2715,62
50	2460,75	2493,33	2521,96	2547,47	2570,42	2591,38	2610,54	2628,21	2644,43	2659,55	2673,64	2686,54	2698,42	2709,48
55	2453,80	2486,37	2515,03	2540,60	2563,64	2584,65	2603,80	2621,50	2637,86	2653,04	2667,14	2679,98	2691,83	2702,98
60	2447,00	2479,56	2508,27	2533,83	2556,90	2577,95	2597,22	2615,03	2631,37	2646,53	2660,62	2673,55	2685,52	2696,73
65	2440,12	2472,75	2501,53	2526,99	2550,22	2571,34	2590,55	2608,37	2624,75	2640,01	2654,04	2667,07	2679,15	2690,32
70	2433,23	2465,88	2494,56	2520,03	2543,32	2564,47	2583,88	2601,67	2617,98	2633,32	2647,52	2660,63	2672,74	2684,02
75	2426,23	2458,78	2487,62	2513,08	2536,39	2557,61	2576,93	2594,80	2611,19	2626,55	2640,81	2653,99	2666,06	2677,34
80	2419,02	2451,67	2480,45	2506,08	2529,24	2550,50	2569,86	2587,93	2604,29	2619,72	2633,99	2647,12	2659,36	2670,69
85	2411,92	2444,63	2473,33	2499,01	2522,29	2543,54	2563,01	2580,93	2597,45	2613,02	2627,39	2640,60	2652,78	2664,16
90	2404,90	2437,62	2466,32	2491,99	2515,28	2536,63	2556,11	2574,02	2590,60	2606,16	2620,52	2633,74	2646,00	2657,41
95	2397,68	2430,33	2459,13	2484,74	2508,10	2529,46	2549,13	2567,03	2583,65	2599,24	2613,57	2626,94	2639,25	2650,63
100	2390,60	2423,22	2452,13	2477,64	2500,91	2522,30	2541,92	2559,96	2576,56	2592,14	2606,50	2619,75	2632,17	2643,75

Heat.	100 grains of spirit to 70 grains of water.	100 grains of spirit to 75 grains of water.	100 grains of spirit to 80 grains of water.	100 grains of spirit to 85 grains of water.	100 grains of spirit to 90 grains of water.	100 grains of spirit to 95 grains of water.	100 grains of spirit to 100 grains of water.	95 grains of spirit to 100 grains of water.	90 grains of spirit to 100 grains of water.	85 grains of spirit to 100 grains of water.	80 grains of spirit to 100 grains of water.	75 grains of spirit to 100 grains of water.	70 grains of spirit to 100 grains of water.	65 grains of spirit to 100 grains of water.
°	Grains.	Grains.	Grains.	Grains.	Grains.	Grains.	Grains.	Grains.	Grains.	Grains.	Grains.	Grains.	Grains.	Grains.
30	2744,20	2753,75	2762,72	2771,08	2778,99	2786,36	2793,22	2799,85	2806,61	2813,85	2821,35	2828,90	2836,39	2844,16
35	2738,13	2747,74	2756,91	2765,32	2773,22	2780,59	2787,54	2794,19	2801,14	2808,52	2816,07	2823,68	2831,36	2839,26
40	2732,24	2741,86	2750,96	2759,50	2767,48	2774,90	2781,84	2788,69	2795,70	2803,17	2810,73	2818,36	2826,31	2834,40
45	2726,09	2735,77	2744,82	2753,36	2761,42	2768,85	2775,94	2782,99	2789,99	2797,45	2805,08	2812,93	2821,00	2829,28
50	2719,93	2729,64	2738,74	2747,27	2755,37	2762,95	2770,14	2777,19	2784,30	2791,72	2799,58	2807,56	2815,71	2824,12
55	2713,60	2723,51	2732,64	2741,24	2749,27	2756,83	2764,09	2771,29	2778,54	2785,96	2793,82	2801,89	2810,23	2818,80
60	2707,40	2717,30	2726,52	2735,17	2743,28	2750,93	2758,17	2765,40	2772,70	2780,26	2788,25	2796,45	2804,85	2813 65
65	2701,05	2710,96	2720,25	2728,98	2737,09	2744,86	2753,21	2759,47	2766,73	2774,43	2782,62	2790,81	2799,38	2808,31
70	2694,76	2704,64	2713,87	2722,75	2730,94	2738,73	2746,06	2753,41	2760,75	2768,45	2776,72	2785,06	2793,80	2802,88
75	2688,14	2698,07	2707,49	2716,35	2724,64	2732,39	2739,89	2747,23	2754,73	2762,58	2770,93	2779,26	2788,00	2797,21
80	2681,50	2691,50	2700,94	2709,76	2718,12	2726,06	2733,53	2740,93	2748,42	2756,43	2764,87	2773,33	2782,14	2791,52
85	2674,95	2684,98	2694,53	2703,33	2711,86	2719,74	2727,25	2734,80	2742,31	2750,22	2758,80	2767,44	2776,33	2785,81
90	2668,29	2678,49	2687,99	2696,91	2705,37	2713,32	2721,01	2728,59	2736,23	2744,24	2752,76	2761,51	2770,59	2780,11
95	2661,51	2671,82	2681,34	2690,33	2698,86	2706,88	2714,61	2722,23	2729,89	2737,98	2746,57	2755,34	2764,57	2774,25
100	2654,76	2664,99	2674,62	2683,63	2692,25	2700,33	2708,04	2715,73	2723,35	2731,55	2740,43	2749,28	2758,48	2768,43

Heat.	60 grains of spirit to 100 grains of water.	55 grains of spirit to 100 grains of water.	50 grains of spirit to 100 grains of water.	45 grains of spirit to 100 grains of water.	40 grains of spirit to 100 grains of water.	35 grains of spirit to 100 grains of water.	30 grains of spirit to 100 grains of water.	25 grains of spirit to 100 grains of water.	20 grains of spirit to 100 grains of water.	15 grains of spirit to 100 grains of water.	10 grains of spirit to 100 grains of water.	5 grains of spirit to 100 grains of water.	Water.	
°	Grains.	Grains.	Grains.	Grains.	Grains.	Grains.	Grains.	Grains.	Grains.	Grains.	Grains.	Grains.	Grains.	
30	2852,03	2859,71	2867,12	2874,43	2881,34	2887,77	2894,22	2900,85	2908,21	2917,19	2928,80	2944,53		
35	2847,45	2855,32	2863,16	2870,87	2878,21	2885,06	2892,07	2899,31	2907,45	2916,95	2928,99	2945,02	2967,14	
40	2842,62	2850,88	2859,06	2867,08	2874,81	2882,30	2889,78	2897,61	2906,39	2916,41	2928,93	2945,25	2967,45	
45	2837,64	2846,16	2854,67	2863,04	2871,22	2879,22	2887,33	2895,67	2904,98	2915,55	2928,49	2945,20	2967,40	
50	2832,76	2841,52	2850,29	2858,96	2867,52	2875,98	2884,57	2893,58	2903,39	2914,42	2927,81	2944,73	2967,05	
55	2827,68	2836,69	2845,72	2854,75	2863,75	2872,67	2881,69	2891,11	2901,42	2913,02	2926,73	2943,98	2966,34	
60	2822,65	2831,90	2841,10	2850,50	2859,87	2869,15	2878,72	2888,62	2899,35	2911,32	2925,50	2942,98	2965,39	
65	2817,49	2826,90	2836,30	2845,97	2855,65	2865,45	2875,49	2885,85	2897,09	2909,43	2923,90	2941,69	2964,11	
70	2812,16	2821,78	2831,61	2841,42	2851,53	2861,63	2872,06	2882,90	2894,56	2907,33	2922,24	2940,13	2962,66	
75	2806,75	2816,63	2826,56	2836,80	2847,14	2857,70	2868,49	2879,67	2891,79	2905,04	2920,17	2938,33	2960,97	
80	2801,25	2811,23	2821,38	2831,92	2842,56	2853,38	2864,54	2876,22	2888,73	2902,35	2917,83	2936,31	2959,07	
85	2795,69	2805,85	2816,32	2827,12	2838,07	2849,28	2860,80	2872,88	2885,56	2899,55	2915,40	2934,14	2956,94	
90	2790,13	2800,40	2811,05	2822,15	2833,38	2844,81	2856,80	2869,16	2882,25	2896,58	2912,84	2931,77	2954,70	
95	2784,36	2794,91	2805,79	2817,08	2828,46	2840,26	2852,47	2865,15	2878,71	2893,44	2910,02	2929,15	2952,08	
100	2778,64	2789,32	2800,25	2811,80	2823,55	2835,30	2848,18	2861,12	2875,07	2890,04	2906,97	2926,28	2949,34	

Real specific gravities at the different temperatures.

Heat.	The pure spirit.	100 grains of spirit to 5 grains of water.	100 grains of spirit to 10 grains of water.	100 grains of spirit to 15 grains of water.	100 grains of spirit to 20 grains of water.	100 grains of spirit to 25 grains of water.	100 grains of spirit to 30 grains of water.	100 grains of spirit to 35 grains of water.	100 grains of spirit to 40 grains of water.	100 grains of spirit to 45 grains of water.	100 grains of spirit to 50 grains of water.	100 grains of spirit to 55 grains of water.	100 grains of spirit to 60 grains of water.	100 grains of spirit to 65 grains of water.
°														
30	,83896	,84995	,85957	,86825	,87585	,88282	,88921	,89511	,90054	,90558	,91023	,91449	,91847	,92217
35	,83672	,84769	,85729	,86587	,87357	,88059	,88701	,89294	,89839	,90345	,90811	,91241	,91640	,92009
40	,83445	,84539	,85507	,86361	,87134	,87838	,88481	,89073	,89617	,90127	,90596	,91026	,91428	,91799
45	,83214	,84310	,85277	,86131	,86907	,87613	,88255	,88849	,89396	,89909	,90380	,90812	,91211	,91584
50	,82977	,84076	,85042	,85902	,86676	,87384	,88030	,88626	,89174	,89684	,90160	,90596	,90997	,91370
55	,82736	,83834	,84802	,85664	,86441	,87150	,87796	,88393	,88945	,89458	,89933	,90367	,90768	,91144
60	,82500	,83599	,84568	,85430	,86208	,86918	,87568	,88169	,88720	,89232	,89707	,90144	,90549	,90927
65	,82262	,83362	,84334	,85193	,85976	,86686	,87337	,87938	,88490	,89006	,89479	,89920	,90328	,90707
70	,82023	,83124	,84092	,84951	,85736	,86451	,87105	,87705	,88254	,88773	,89252	,89695	,90104	,90484
75	,81780	,82878	,83851	,84710	,85493	,86212	,86864	,87466	,88018	,88538	,89018	,89464	,89872	,90252
80	,81530	,82631	,83603	,84467	,85248	,85966	,86623	,87228	,87776	,88301	,88781	,89225	,89639	,90021
85	,81283	,82386	,83355	,84221	,85006	,85723	,86380	,86984	,87541	,88067	,88551	,88998	,89409	,89793
90	,81039	,82142	,83111	,83977	,84762	,85483	,86139	,86743	,87302	,87827	,88312	,88758	,89173	,89558
95	,80788	,81888	,82860	,83724	,84511	,85232	,85896	,86499	,87060	,87586	,88069	,88521	,88937	,89322
100	,80543	,81643	,82618	,83478	,84262	,84984	,85646	,86254	,86813	,87340	,87824	,88271	,88691	,89082

Heat.	100 grains of spirit to 70 grains of water.	100 grains of spirit to 75 grains of water.	100 grains of spirit to 80 grains of water.	100 grains of spirit to 85 grains of water.	100 grains of spirit to 90 grains of water.	100 grains of spirit to 95 grains of water.	100 grains of spirit to 100 grains of water.	95 grains of spirit to 100 grains of water.	90 grains of spirit to 100 grains of water.	85 grains of spirit to 100 grains of water.	80 grains of spirit to 100 grains of water.	75 grains of spirit to 100 grains of water.	70 grains of spirit to 100 grains of water.	65 grains of spirit to 100 grains of water.
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30	,92563	,92889	,93191	,93474	,93741	,93991	,94222	,94447	,94675	,94920	,95173	,95429	,95681	,95944
35	,92355	,92680	,92986	,93274	,93541	,93790	,94025	,94249	,94484	,94734	,94988	,95246	,95502	,95772
40	,92151	,92476	,92783	,93072	,93341	,93592	,93827	,94058	,94295	,94547	,94802	,95060	,95328	,95602
45	,91937	,92264	,92570	,92859	,93131	,93382	,93621	,93860	,94096	,94348	,94605	,94871	,95143	,95423
50	,91723	,92050	,92358	,92647	,92919	,93177	,93419	,93658	,93897	,94149	,94414	,94683	,94958	,95243
55	,91502	,91837	,92145	,92436	,92707	,92963	,93208	,93452	,93696	,93948	,94213	,94486	,94767	,95057
60	,91287	,91622	,91933	,92225	,92499	,92758	,93002	,93247	,93493	,93749	,94018	,94296	,94579	,94876
65	,91066	,91400	,91715	,92010	,92283	,92546	,92794	,93040	,93285	,93546	,93822	,94099	,94388	,94689
70	,90847	,91181	,91493	,91793	,92069	,92333	,92580	,92828	,93076	,93337	,93616	,93898	,94193	,94500
75	,90617	,90952	,91270	,91569	,91849	,92111	,92364	,92613	,92865	,93132	,93413	,93695	,93989	,94301
80	,90385	,90723	,91042	,91340	,91622	,91891	,92142	,92393	,92646	,92917	,93201	,93488	,93785	,94102
85	,90157	,90496	,90818	,91119	,91403	,91670	,91923	,92179	,92432	,92700	,92989	,93282	,93583	,93902
90	,89925	,90270	,90590	,90891	,91177	,91446	,91705	,91962	,92220	,92491	,92779	,93075	,93381	,93703
95	,89688	,90037	,90358	,90662	,90949	,91221	,91481	,91740	,91998	,92272	,92562	,92858	,93170	,93497
100	,89453	,89798	,90123	,90428	,90718	,90992	,91252	,91513	,91769	,92047	,92346	,92646	,92957	,93293

Heat.	60 grains of spirit to 100 grains of water.	55 grains of spirit to 100 grains of water.	50 grains of spirit to 100 grains of water.	45 grains of spirit to 100 grains of water.	40 grains of spirit to 100 grains of water.	35 grains of spirit to 100 grains of water.	30 grains of spirit to 100 grains of water.	25 grains of spirit to 100 grains of water.	20 grains of spirit to 100 grains of water.	15 grains of spirit to 100 grains of water.	10 grains of spirit to 100 grains of water.	5 grains of spirit to 100 grains of water.	Water.	
°														
30	,96209	,96470	,96719	,96967	,97200	,97418	,97635	,97860	,98108	,98412	,98804	,99334		
35	,96048	,96315	,96579	,96840	,97086	,97319	,97556	,97801	,98076	,98397	,98804	,99344	1,00090	
40	,95879	,96159	,96434	,96706	,96967	,97220	,97472	,97737	,98033	,98373	,98795	,99345	1,00094	
45	,95705	,95993	,96280	,96563	,96840	,97110	,97384	,97666	,97980	,98338	,98774	,99338	1,00086	
50	,95534	,95831	,96126	,96420	,96708	,96995	,97284	,97589	,97920	,98293	,98745	,99316	1,00068	
55	,95357	,95662	,95966	,96272	,96575	,96877	,97181	,97500	,97847	,98239	,98702	,99284	1,00038	
60	,95181	,95493	,95804	,96122	,96437	,96752	,97074	,97409	,97771	,98176	,98654	,99244	1,00000	
65	,95000	,95318	,95635	,95962	,96288	,96620	,96959	,97309	,97688	,98106	,98594	,99194	,99950	
70	,94813	,95139	,95469	,95802	,96143	,96484	,96836	,97203	,97596	,98028	,98527	,99134	,99894	
75	,94623	,94957	,95292	,95638	,95987	,96344	,96728	,97086	,97495	,97943	,98454	,99066	,99830	
80	,94431	,94768	,95111	,95467	,95826	,96192	,96568	,96963	,97385	,97845	,98367	,98991	,99759	
85	,94236	,94579	,94932	,95297	,95667	,96046	,96437	,96843	,97271	,97744	,98281	,98912	,99681	
90	,94042	,94389	,94748	,95123	,95502	,95889	,96293	,96711	,97153	,97637	,98185	,98824	,99598	
95	,93839	,94196	,94563	,94944	,95328	,95727	,96139	,96568	,97025	,97523	,98082	,98729	,99502	
100	,93638	,93999	,94368	,94759	,95152	,95556	,95983	,96424	,96895	,97401	,97969	,98625	,99402	

When all the experiments had been completed, and the tables here given were just brought into order, an ingenious member of the Royal Society, scarcely less celebrated for his theoretical knowledge than his skill as an artist, published a pamphlet containing censures on our first experiments, and proposing other methods, as much superior to those we had adopted.* In drawing up the Report, in order to avoid prolixity, the reasons for choosing some of the methods were not given, where they did not seem likely to be a subject of controversy; but this pamphlet makes it necessary to assign the motives of our preference, that the public may judge how far we are justified.

First, as to the proportions of the mixtures; which were made by taking an equal quantity of spirit in every instance, and adding to it successively larger quantities of distilled water, as far as to an equal weight; with the intention of going through the watery mixtures on the same plan. This was done for the following reasons: 1. Because it was thought more likely to avoid blunders, if the quantity of only one of the ingredients was changeable, that the operator might not have his attention distracted with computing and weighing out two different quantities for each mixture. 2. Because by this progression the experiments come closer together about the medium degrees of strength, where it was supposed most accuracy would be wanted for practice. 3. As it was thought, from the first, that the best method of adjusting the duty would be by the absolute quantity of alcohol in any mixture, rather

* An account of Experiments to determine the Specific Gravity of Fluids; by J. Ramsden. London, 1792.

than the proportion *per cent.*, or the strength above or under proof, we judged it most expedient not to make the mixtures on either of the two last mentioned principles, lest an undue bias should be given to the judgment, merely from the mode of conducting the experiments. No real difficulty can arise in forming tables of any kind out of these numbers, which answer to an harmonic progression of strength. If the operation be tedious, to obtain the specific gravity of any single proportion, *per cent.* or otherwise, of alcohol and water, the trouble of reducing the whole to a table would not be great, and when once executed, it is done for ever.

Secondly, though the chief reasons for making the mixtures by weight, rather than by measure, have been already assigned in the Report, it is now proper to add something further on that subject. Nothing but arithmetic is required for obtaining the proportions by measure with the utmost exactness; and, as in the former case, though the operation be a little laborious singly, the computation of the entire table will be sufficiently easy. Such a table was recommended in the Report, and can be constructed by any person tolerably conversant with figures. In the pamphlet mentioned above, a method is recommended for proportioning the mixtures by measure, while the actual quantity of spirit is determined by weight, at one operation. The idea is ingenious, but in the execution it seems liable to the following objections. 1. The difficulty of obtaining the full penetration of the spirit and water, in a vessel of the shape required, where, by the intervention of such a narrow neck as is wanted, the free agitation of the fluid must be greatly impeded. 2. The difficulty of getting out all the air-bubbles, produced by the shaking, &c. in a vessel so shaped.

3. The difficulty, or almost impossibility, of bringing the mixture, by the repeated fillings, to coincide exactly with the ring on the neck: for this purpose, the last quantities of water must be put in by such small portions at a time, that scarcely any attention will be equal to the task; and if at length too much be added, it cannot be taken out again without injuring, in some degree, the accuracy of the experiment, which depends on combining the precise quantity of water required to fill the vessel up to the mark, when the full penetration has taken place. 4. In opening the vessel so frequently, to fill it up, a sensible part of the spirit must be lost by evaporation. 5. Moreover it is necessary that, at the end of the operation, the fluid should throughout be exactly of the same temperature as the pure spirit was in the preparatory experiment with it alone; the difficulty of effecting and determining which must be obvious to every one, especially in a vessel of such a size and shape. Lastly, as this vessel is much less manageable than the weighing-bottle, I think the fluid in it cannot be brought to the mark with nearly the same degree of accuracy. These objections, joined to the consideration that no object can be attained in this way, which was not accomplished, with at least equal accuracy, and probably no greater trouble, by weighing the spirit and water separately, determined us not to attempt any experiments with such an instrument.

Thirdly, it is now to be explained why we undertook to determine the effect of heat and cold on the fluids, by means of the weighing-bottle. When, preparatory to our former experiments, that part of the subject came under consideration, the method of ascertaining the expansions and contractions, by means of instruments like thermometers, was one of the first

that presented itself. On this occasion, Mr. CAVENDISH was so good as to mention some experiments made by his father, Lord CHARLES CAVENDISH, with instruments on that construction, for the very purpose of determining the expansion of fluids; and other experiments, of the same nature, have appeared in print. The application of this method, however, was thought liable to a most important objection, from the great difficulty there is of being sure that the spirits in the ball are exactly of the temperature indicated by the thermometer placed by the side of it. To enlarge upon this circumstance would be useless, as every person accustomed to experiments is aware, that all possible precautions, joined to the utmost attention in the observer, are scarcely sufficient to ensure this essential correspondence of temperature; which reason alone would have induced us to prefer the method by weight. But there was another argument which still more forcibly determined us in favour of the latter; namely, that the effect of mixture was found in that way, and therefore we were sure it admitted of as great accuracy as was obtained in the other part of the experiments. Greater nicety, if there had been a method which allowed of it, would have been superfluous; and to incur the risk of less accuracy would have been absolutely unjustifiable. By using the same method to determine all the changes of specific gravity, those from heat as well as those from mixture, an uniformity is given to the whole series of experiments, and no one part of the results is liable to more suspicion than another.

Till this time, I believe, the instruments with a ball and tube, for trying expansions, had all been constructed in the manner of real thermometers, to be filled by means of heat;

which circumstance, and the trouble attending it, was a further objection to their use: but in the pamphlet abovementioned are proposed two instruments of this nature, to be filled without heat; one being provided with two equal tubes, the other with a short tube, closed by a stopper. Though both these instruments, and especially the latter, seemed liable to several causes of error, yet, to remove doubts, and bring the method by weight to a proper test, Mr. GILPIN was desired to make some trials with them; Mr. RAMSDEN, the author of the pamphlet, having been previously requested to go through the whole series of experiments on his own plan, which he declined to do. With no small difficulty Mr. GILPIN got the instruments executed; and an account of the experiments to which he subjected them shall be given, in his own words, at the end of this Report. From the perusal of that account, it will be perceived, that the disagreement of the experiments among themselves, is nearly equal to the quantity by which any of them differ from the expansions as obtained by weight. On the whole, however, they give the expansion somewhat less, the cause of which I do not see; possibly it may depend on the fluid in the ball not being quite heated and cooled to the degree shown by the accompanying thermometer; possibly there may be a difference in the expansion of the glass with which the instruments were made, and that of the weighing-bottle, for these numbers are in both cases the excess of the expansion of the fluid over glass; or it may turn on some other circumstance, which has eluded our attention. Whatever may have occasioned the deficiency, I think the experiments will satisfy any one, that most dependence is to be placed on the weights; and at all events the difference is not such

as to affect the third place of decimals, or consequently the tables intended for practice.

Probably no one will be surprised that we did not think it necessary to make trial of the weighing-bottle proposed by Mr. RAMSDEN. Not to mention other inconveniences attending this instrument, it seems evident that a piece of flat glass, with a thermometer projecting from it, laid down on the mouth of a bottle, cannot be depended upon to push off the superfluous liquor equally every time; and that the proper wiping of the bottle, when so covered, will be attended with difficulties of various kinds.

It is true that the experiments by weight took up much time, and demanded great patience. But I believe that similar experiments, by the methods recommended in the pamphlet, if executed with the same degree of accuracy, would be found not much less tedious. However this may be, it is a consideration of no consequence, provided the results at length obtained be right. Now of these there is no direct impeachment, though some doubts are thrown upon them, on four accounts; evaporation; condensation of moisture on the weighing-bottle; difficulty of shaking the fluid in it; and uncertainty in determining the heat. With regard to evaporation, its effect, we hope, has been ascertained, and allowed for, in these new experiments. All error from condensation of moisture was obviated by careful wiping. The fluid in the weighing-bottle was agitated, and mixed together, by means of the thermometer immersed in it; besides which, a considerable degree of motion could be given to it, even when the ball was very nearly full, by shaking the bottle in various directions. Mr. GILPIN's known accuracy, and the care he bestowed on

these experiments, must gain him credit for having duly watched the thermometer, so as to seize the moment when it gave the just temperature of the mass.

Our experiments were finished, and the tables now given were drawn out, before the appearance of Mr. RAMSDEN's pamphlet. Yet if any of the methods he proposed had been really preferable, the whole series should have been repeated on that new plan, and particularly with regard to the effect of heat, if the instruments for that purpose had been found to answer the character given of them. But as this was not the case, we have thought it right to adhere to an obvious and direct method, in which, however laborious, there can be no fallacy, and the uniformity of which ensures an equal degree of accuracy to every part of the operation.

SINCE the publication of our first tables, several hydrometers have been contrived, with the view of applying them to practice. Those of copper were rejected on account of the errors which small and almost imperceptible bruises in them might occasion; and for the same reason no other metal was tried. Mr. GILPIN has constructed two areometers of glass; one with the stem so divided, that an easy table may be formed for the correction necessary according to the different weights with which it is used; the other with a separate scale fixed to each of those weights, made to slip into the tubular stem of the instrument; a contrivance that obviates the necessity of a table. Mr. RAMSDEN also has invented a balance hydrometer, with several varieties of construction, one of which is detailed in his pamphlet. All the above-mentioned instruments appear to have fully as much accuracy as can be

required; of their preference in other respects, the practical officers who are to use them will probably be found the best judges. That which can be managed with the greatest facility and quickness, which affords the least opportunity of making blunders, which is least liable to be out of order, and shews most immediately if it be so, will unquestionably prove the most satisfactory in practice. Hydrometers having a thermometer inclosed within them must be condemned, as not ascertaining the temperature with the requisite precision. An attempt to supersede the use of the thermometer, by employing for the hydrometer, a substance which "has the same "degree of expansion as the mean of the compounds," is very inconsistent with the kind of accuracy sought by these experiments.

As an allowance is made, in our table of specific gravities, for the expansion and contraction of the glass weighing-bottle, this must be taken into the account, with every areometer, whenever much exactness is desired.

I am still of opinion, that the best way of laying the duty would be directly on the quantity of alcohol contained in any composition; and though this might require too great a change in the present system of laws, yet as the same principle may be applied in estimating the strength, and taking stock, I will just mention in what manner the computation can be most readily made. From the numbers in this Supplementary Report a table must be constructed, on the top of which shall stand every degree of heat from 40, or 30, to 80, and at the side every specific gravity from ,825 to 1,000, if it be thought necessary, or as much less as will answer the purpose. The places of this table are to be filled up, by computing, from

the original tables, the quantity by measure of alcohol and water corresponding to each specific gravity and degree of heat; and then dividing the quantity of alcohol by the whole quantity of the mixture; thus a decimal multiplier will be obtained, which must be put in the compartment of the table formed by the intersection of the columns of that particular heat and specific gravity. When the table is completed in this manner, we have only to multiply the contents of any cask, as found on gauging it, by the decimal number given in the table for the heat and specific gravity of the liquor, and the product will be the quantity of pure alcohol it contains. Hence it must be evident, that no objection can lie to this method on account of difficulty; if, however, it be thought more eligible, for different reasons, to adopt the proportion of alcohol *per cent.*, the relation of strength to the point of proof, or any other method, the numbers in this Report will equally apply to all, with the proper variation in the table to be employed.

As to the calculation necessary for constructing the table of decimal multipliers, what has been already said with respect to the reduction of the harmonic numbers applies also to it. The labour of the whole will not be very great, and it is once for all. The process is not an approximation, but a plain arithmetical computation, which may be carried on true to as many decimal places as the experiments will allow. For this purpose, indeed, it is necessary to have the weight of a known measure of water. Mr. RAMSDEN's method of obtaining this, by means of a cylinder, is far preferable to that of hollow cubes, particularly if the ends of the cylinder can be made as true as the body of it. But in applying this instrument to fix the term of proof, as proposed by that gentleman, it must

be remembered, that 7lb. 13 oz. is not the weight of a gallon of proof spirit, but of spirit one to six under proof. On that proportion the value of proof was computed in the Report, by the same rule as Mr. RAMSDEN has since given, but which it was not thought necessary to detail at full length.

Although, as was said in the original Report, the quantity of extraneous substances usually found in spirituous liquors does not increase their specific gravity so much as to be worth the consideration of Government, yet this is by no means the case when such substances are added intentionally. The effect of alkalies is well known. Mr. RAMSDEN's experiments shew how great a change of specific gravity is produced by sugar, when dissolved plentifully in weak liquors; and in an experiment made by order of the Board of Excise, $\frac{5}{64}$ part of sugar, put into very strong spirit, reduced its apparent strength no less than 17 *per cent.* by CLARKE's hydrometer.

I conclude with observing, that the *execution* of the experiments, and of the computations, rested entirely with Mr. GILPIN, who is responsible for their accuracy, and entitled to the praise they may be found to merit. For the general plan, as well as the particular methods adopted, I hold myself accountable, and have now so fully stated my reasons for what I recommended to be done, that any competent person will readily judge of their validity. In this and the foregoing Report, I have purposely avoided all philosophical deductions, and a comparison with former experiments; that the narrative might not be loaded with any foreign matter, to interfere with the practical object for which this business was undertaken.

C. BLAGDEN.

Appendix to the foregoing Report. In a Letter from Mr. George Gilpin, *Clk. R. S. to* Charles Blagden, *M. D. Sec. R. S.*

SIR,

HAVING completed two instruments for trying the expansion of fluids, according to the method described by Mr. RAMSDEN, with a stopper going into a tube on the side of the ball, I now present you with an account of the experiments which I have made with them, that you may judge how far such instruments are deserving of notice. The scale of the longest admits of ,26 of an inch for each degree of the thermometer, and that of the shortest ,17 of an inch for each degree.

They were charged with pure spirit: some of the same that was made use of in our experiments by weight, specific gravity ,82514: and having hung them up by the side of each other to a piece of wood, provided for the purpose, with the same sensible thermometer hanging between them that was used in our experiments by weight, I immersed them in a large quantity of water brought to the temperature of 60°, the one quite, the other nearly, to the height of the fluid in the stem. In this water they were suffered to continue until they had arrived at that temperature, when it was observed that the spirit in the tube of the long instrument stood at 0, or the commencement of the scale, and the spirit in the tube of the short instrument stood at $\frac{15}{10000}$ above 0, which I shall

in future for shortness call 1,5, and which it is evident must be applied with the sign + or — to the quantity of expansion or contraction read off from above or below 0, as the case may require. They were then cooled down to 30° of temperature, when the spirit in the long instrument was found to stand 165, and that in the short instrument 163,5 below 0. They were again brought to the temperature of 60°, when the spirit in the long instrument was found to stand 0,5, below 0, and in the short instrument 1,5 above 0 as before. They were then heated to 100°, when the spirit in the long instrument stood at 231, and in the short instrument at 233,5 above 0. I brought them again to the temperature of 60°, when the spirit in the long instrument was found to stand 3 below 0, but in the short instrument 1,5 above 0 as before.

It appears from these experiments, that the contraction of the spirit, by the long instrument, for 30 degrees, that is, in cooling down from 60° to 30°, is 165; but in heating it up again to 60°, it was found not to stand at 0, as before, but 0,5 below; therefore the expansion will be only 164,5, the mean is 164,75. The expansion, on heating up from 60° to 100°, will be 231+0,5 = 231,5; but on cooling down from 100° to 60° again, the spirit was found to sink 3 below 0, therefore it will be 231+3 = 234; the mean 232,75, and the total expansion, from 30° to 100° = 397,5; differing from ours, in defect, by 0,05 of a division. But the two methods of heating from 60° to 100°, and cooling from 100° to 60° again, differ 2,5 divisions, or so many ten thousandth parts of the whole.

The contraction from 60° to 30°, by the short instrument, appears to be 163,5+1,5 = 165, and the expansion, on heating

up again to 60°, the same; from 60° to 100° it was found to be 233,5 − 1,5 = 232, and the contraction in cooling down again from 100° to 60° the same; the total expansion 397, differing from ours 0,55 of a division, in defect.

After the above experiments, the instruments were emptied of the spirit; and another day, preparatory to a repetition of the experiments, they were charged again with some of the same spirit that was used before, and the results found to be as follow.

Having brought them to the temperature of 60°, I found the spirit in the long instrument to stand 3 above 0, and in the short instrument 5 below 0. They were then cooled down to 30° of temperature; when the spirit in the long instrument was found to sink to 161,5, and in the short instrument to 167,5. They were afterwards brought back again to 60° of temperature; when the spirit in the long instrument stood 3 above 0, as before, but in the short instrument 5,5 below 0. I then heated them up to 100°, and it stood in the long instrument at 234, and in the short instrument at 226, above 0. They were again brought to the temperature of 60°; when it was found to stand in the long instrument at 0, and in the short instrument at 8 below 0.

From the above experiments it appears, that the contraction by the long instrument, in cooling down from 60° to 30°, is 161,5 + 3 = 164,5, and the expansion in heating up again to 60°, the same. In heating up from 60° to 100°, 234 − 3 = 231; but the contraction in cooling down again from 100° to 60°, 234; the mean is 232,5, and the total expansion from 30° to 100° = 397, differing from the experiments by weight 0,55 of a division, in defect: but if no mean be taken, the deficiency

will appear greater. The difference between heating up from 60° to 100°, and cooling down again from 100° to 60°, is 3 divisions, still more considerable in this than in the last experiment.

The contraction by the short instrument from 60° to 30° is 167,5 — 5 = 162,5, and the expansion from 30° to 60° again 167,5 — 5,5 = 162; the mean is 162,25. On heating up from 60° to 100°, 226 + 5,5 = 231,5; but the contraction, in cooling down again from 100° to 60° was 226 + 8 = 234; the mean is 232,75, and the total expansion from 30° to 100° = 395; differing from the experiments by weight 2,55 divisions, in defect. The difference between heating up from 60° to 100°, and cooling down again from 100° to 60°, is 2,5 divisions.

After the above experiments had been made, the spirit was let out, and, on a subsequent day, the two instruments were charged again with some of the same spirit, previous to the following experiments.

After bringing the spirit to the temperature of 60°, I found it to stand in the long instrument 6 above 0, and in the short instrument 2 below 0. I cooled the spirit down to 30°, when it stood in the long instrument 158,5, and in the short instrument 166, below 0. I brought it again to the temperature of 60°, and it returned to the same point it set off from, in both instruments. The spirit was then heated to 100°; when it rose in the long instrument to 235, and in the short instrument to 230, above 0. I cooled it again to the temperature of 60°, when it was found to stand in the long instrument 1 below 0, and in the short instrument 5 below 0.

It appears, from the above experiments, that the contraction by the long instrument in cooling down from 60° to 30° is 158,5

$+6 = 164{,}5$, and the expansion in heating up from 30° to 60°, the same. On heating up to 100°, $235-6 = 229$, but the contraction in cooling down from 100° to 60° again, $235+1 = 236$; the mean is 232,5, and the total expansion from 30° to 100° = 397,0; differing from the experiments by weight 0,55 of a division, in defect; but the two methods in this experiment differ very considerably from each other, namely, by no less a quantity than 7 divisions. In this experiment it seems probable either that some of the spirit leaked out at the stopper, or that the stopper shifted its place a little, so as to enlarge the capacity of the ball.

The contraction by the short instrument in cooling down from 60° to 30° was $166 - 2 = 164$, and the expansion on heating up again to 60°, the same. On heating up to 100°, it was $230 + 2 = 232$, but on cooling down again to 60° the contraction was $230 + 5 = 235$; the mean is 233,5, and the total expansion 397,5; differing from the experiments by weight 0,05 of a division, in defect. The difference between the two methods, in heating up from 60°, to 100°, and in cooling down again from 100° to 60°, is three divisions.

It appears from the preceding experiments, that the mean of all the quantities found on heating up from 30° to 100°, and cooling down from 100° to 30°, taken together, gives for the total expansion 397,16 by the long instrument, and 396,5 by the short; the former errs 0,39, and the latter 1,05 divisions from the experiments by weight, in defect. It appears also that the mean of all the quantities found by the long instrument, on heating up from 30° to 100°, gives for the total expansion 4,34 divisions less than the mean of all the quantities taken together, by the same instrument, on cooling down from

100° to 30°; and the difference by the short instrument is 2 divisions.

The following experiments were made with a mixture of equal parts of spirit and water by weight; the spirit being of the strength already mentioned.

Having charged the instruments with the mixture, and brought it to the temperature of 60°, it was found to stand in both of them at 1 above 0. The mixture was then cooled down to 30°, when it stood at 125 below 0 in the long instrument, and 124,5 in the short instrument. It was brought back to the temperature of 60°, when, in the long instrument it was found to stand 1,5 above 0, but in the short instrument 1 above 0 as before. I heated the mixture to 100°, when it stood at 185 in the long instrument, and in the short instrument at 183,5 above 0. The mixture was afterwards cooled to the temperature of 60°, when it was found to stand 2,5 above 0 in the long instrument, but in the short instrument 1,5 below 0. After keeping them upright in the temperature of 60° two hours, I found the mixture in the long instrument to stand 3 above 0, but in the short instrument 2 below 0. I heated it again to 100°, when the mixture in the long instrument was found to stand 185, and in the short instrument 180,5 above 0. I brought the mixture again to the temperature of 60°, and found it stand 2,5 above 0 in the long instrument, and in the short instrument 4 below 0.

From the above experiments the contraction of this mixture from 60° to 30° was found to be, by the long instrument, 125 + 1 = 126; but in heating up to 60° again, the expansion was 125 + 1,5 = 126,5; the mean is 126,25. The expansion in heating up from 60° to 100° was 185 — 1,5 = 183,5,

but the contraction in cooling down from 100° to 63° was 185 — 2,5 = 182,5. In heating up again to 100° it was 185 — 3 = 182, but in cooling down again to 60°, 185 — 2,5 = 182,5; the mean of the four is 182,62, and the total expansion from 30° to 100° = 308,87; differing from the experiments by weight 1,7 division, in defect. The difference between the two methods of heating up from 60° to 100°, and cooling down again from 100° to 60°, taking a mean of the two heatings, and the mean of the two coolings, is 0,75 of a division.

The contraction by the small instrument, in cooling down from 60° to 30°, was 124,5 + 1 = 125,5. On heating up again to 60°, the expansion was the same. In heating up from 60° to 100° the expansion was 183,5 — 1 = 182,5; but in cooling down to 60° again, the contraction was 183,5 + 1,5 = 185. In heating again up to 100°, the expansion was 180,5 + 2 = 182,5; but in cooling again to 60° the contraction was 180,5 + 4 = 184,5. The mean of these four gives 183,62, for the expansion from 60° to 100°; and therefore the total expansion from 30° to 100° will be 309,12, differing from the expansion found by the experiments by weight 1,45 division, in defect. The difference between the mean of the two heatings up from 60° to 100°, and the two coolings down from 100° to 60° again, is 2,75 divisions.

The mixture made use of in the above experiment was now emptied out, and the instruments were charged with more of the same, preparatory to the following experiments.

The mixture being brought to the temperature of 60°, was found to stand in each of the instruments at 1,5 above 0. It was then cooled down to 30°, and it stood in the long instru-

ment 124,5, and in the short instrument 125, below 0. It was then brought back to the temperature of 60°, and it stood in the long instrument 1,5 above 0 as before, but in the short instrument 1 above 0 only. I heated the mixture to 100°, when it stood at 182,5 in the long instrument, and in the short instrument at 183,5 above 0. The mixture was afterwards brought to the temperature of 60°, when it was found to stand at 0 in both instruments.

It appears from the preceding experiments, that the contraction of this mixture in cooling down from 60° to 30°, by the long instrument is $124,5 + 1,5 = 126$, and the expansion in heating up again from 30° to 60°, the same. The expansion in heating up from 60° to 100° was $182,5 - 1,5 = 181$; but in cooling down again from 100° to 60°, the contraction was 182,5; the mean is 181,75, and the total expansion from 30° to 100° will be 307,75, differing from the experiments by weight 2,82 divisions, in defect. The difference between the two methods of heating up from 60° to 100°, and cooling down again from 100° to 60°, is 1,5 division.

The contraction of the mixture in cooling down from 60° to 30°, by the short instrument is $125 + 1,5 = 126,5$; but in heating up again to 60°, the expansion was $125 + 1 = 126$; the mean is 126,25. The expansion in heating up from 60° to 100° was $183,5 - 1 = 182,5$, but in cooling down again from 100° to 60° the contraction was 183,5; the mean is 183; and therefore the total expansion from 30° to 100° will be 309,25, differing from the experiments by weight 1,32 division, in defect. The difference between the two methods of heating up from 60° to 100°, and cooling down again from 100° to 60°, is 1 division.

It appears from all the preceding experiments with the mixture of equal parts of spirit and water, that the mean of all the quantities found on heating up from 30° to 100°, and cooling down again from 100° to 30°, taken together is, for the total expansion, 308,46 by the long instrument, and by the short instrument 309,29; the former errs 2,11, and the latter 1,28 divisions, in defect, from the experiments by weight; and that the mean of all the quantities found by the long instrument, in heating up from 30° to 100°, gives for the total expansion 0,33 division less than the mean of all the quantities taken together, by the same instrument, in cooling down from 100° to 30°. The difference by the short instrument is 1,83 division.

Although the results found from the preceding experiments come nearer those of the experiments by weight than might have been expected, considering the many objections that instruments of this kind must naturally present, and the great differences which were actually found among themselves on repeating the experiments, especially in the expansion of pure spirit, where the difference has been equivalent to 1,68 grains in weight, upon the quantity used in our experiments with the weighing-bottle; yet I think, after a careful perusal of the foregoing facts, I shall not be thought too precipitate when I say, that these instruments neither do nor can possess that accuracy which we have been led to expect from them. We have seen, in the foregoing experiments, that there has sometimes been apparently a loss of some part of the fluid, after an alteration of the temperature; at other times there appeared to be no loss at all; and sometimes there appeared to be even more spirit in the instrument than there was at

first. These contradictory facts may, I apprehend, be accounted for in the following manner. The mechanical operation of grinding a stopper that will fit so delicate a tube, as is here necessary, perfectly tight, must be acknowledged to be difficult; and should it happen to be done accurately, so that none of the fluid is lost in one degree of temperature, it is very doubtful whether, on exposing this instrument to a different temperature, the expansion would be the same in both the tube and stopper. It appears most probable, from these experiments, that they actually did not expand alike, as perhaps no two pieces of glass ever do; and the effect to be expected from a less expansion of the stopper than of the tube is, either that some of the fluid would leak out, or that the capacity of the ball would be enlarged. But the chief reason why there may sometimes seem to be a loss, at other times no loss at all, I apprehend to be, that more of the fluid will adhere to the upper part of the tube, on filling it, at one time than at another. In the use of this instrument also, a small error may arise from the stopper not being always put in exactly alike; in which case the capacity of the instrument would be altered; and, of course, the divisions on its stem would not give the expansion of the fluid accurately. Care was always taken in these experiments to put the stopper in as nearly alike as possible; but it might not perhaps always be done exactly so.

It is also obvious, that experiments with this instrument will be affected by another source of error, if made in the manner which is recommended, namely, by heating the fluid up from 60° to 100°, and cooling it down again to 30°: for it must be evident that the whole length of the tube will then be left wet by the fluid, in sinking from 100° to 30°, and conse-

quently the expansion will be made to appear too great. The effect of this circumstance will be very considerable; but in the use of this instrument we have no certain means of ascertaining with accuracy the quantity of error occasioned by it, because that quantity falls in with other errors. Of this I shall speak more hereafter, in my remarks on another instrument which I constructed, without a stopper, and which was also recommended by the same gentleman.

The former experiments were all made with that kind of instrument which has a tube rising from the side of the ball, with a ground-glass stopper inserted into it; an instrument we have seen by no means to be considered as sufficiently accurate for ascertaining the expansion of fluids; I therefore constructed another, similar to the other of the two recommended by Mr. Ramsden, which has two tubes rising from the ball, one on each side.

Having charged this instrument with some of the same spirit which was made use of in the former experiments, and brought it to the temperature of 60°, the spirit in the two tubes was found to stand at 4 above 0. It was then cooled down to 30°, when the spirit in the two tubes was found to stand at 161 below 0, the instrument being always so held as to bring it to the same point in both tubes. I then heated it up to 100°, and it stood in the two tubes at 296 above 0. I cooled it again to 30°, when it was found to stand in the two tubes at 162 below 0. It was again heated up to 100°, and it stood in the two tubes at 296 above 0. I then brought it again to the temperature of 60°, and found it to stand in the two tubes at no more than 3 above 0.

It appears from the above experiments, that the contraction of the spirit from 60° to 30° is 161 + 4 = 165, and the expansion in heating up again to 60°, the same. On heating up from 60° to 100°, 236 — 4 = 232, and therefore the total expansion from 30° to 100° is 397; but in cooling down from 100° to 30°, the total expansion will be 236 + 162 = 398; the former quantity differs 0,55, in defect, and the latter 0,45 of a division, in excess, from the experiments by weight. Now it is evident that the method of heating up from 30° to 100° can only be admitted as giving a true result, for it was found on cooling the spirit down from 100° to 30°, that the contraction from 60° to 30° had been increased by 1; so much of the fluid being left behind in the upper part of the two tubes, as appears on its being heated up again to 60°, for then it stood lower in the two tubes by 1 than it did before; care having been taken that the upper part of the two tubes should be as dry as possible before the experiment commenced, for which purpose the instrument was charged over night, and constantly kept in a vertical position. It must also be obvious that 397, which was found for the total expansion in heating up from 30° to 100°, must likewise be too great by nearly the same quantity, it having been cooled down from 60° to 30° previous to its being heated up to 100°, as this would tend to make it sink so much lower than it would have done in the first instance had that not been the case.

After the above experiments, the spirit was poured out, and, previous to the repetition of the foregoing experiments on a future day, it was charged with more of the same spirit which was used in the former experiments, and the instrument was hung up as before.

Having brought it to the temperature of 60°, the spirit in the two tubes was found to stand at 4 above 0. I cooled it down to 30°, when it stood in the two tubes at 160,5 below 0. I brought it again to the temperature of 60°, and it was found to stand in the two tubes at 4 above 0 as before. It was then heated up to 100°, and was found to stand in the two tubes at 236 above 0. I cooled it down again to 30°, and found it to stand in the two tubes at 162 below 0. It was then brought again to the temperature of 60°, and was found to stand in the two tubes at no more than 2,5 above 0.

From the preceding experiments it appears, that the contraction in cooling down from 60° to 30°, is $160,5 + 4 = 164,5$, and in heating up from 30° to 60° again, the expansion was the same. In heating up from 60° to 100°, the expansion was $236 - 4 = 232$, therefore the total expansion in heating up from 30° to 100° will be 396,5; but in cooling down again from 100° to 30°, we shall have for the total expansion $236 + 162 = 398$. The former quantity of 396,5 differs 1,05 in defect, and the latter 0,45 of a division in excess, from the experiments by weight: but it is obvious from this, as well as from the preceding experiment, that the method of heating up from 30° to 100° can only give the true expansion, as has already been observed: for when the spirit is cooled down from 60° to 30° the expansion will be made greater than it ought to be; as it was found on setting off, that the spirit in the two tubes at 60° of temperature stood at 4 above 0; and after having been cooled down to 30°, and heated up again to 60°, it was found to stand the same: but after having been heated up to 100°, and cooled down again to 30°, the contraction

from 60° to 30° was found greater by 1,5 than before; and on heating up to 60° again, it was found to stand only 2,5, instead of 4, above 0. It is therefore very reasonable to conclude, that a quantity equal to 1,5 had adhered to the upper part of the tubes, and no well-grounded objection can be made to this, when we consider that 1 division is only equal to ,015 of a grain of spirit, in this instrument.

It appears then from the preceding experiments, that the mean of the quantities found, on heating up from 30° to 100°, including the error that must arise from some of the fluid adhering to the tube, in cooling it down from 60° to 30°, previous to its being heated up from 30° to 100°, gives for the total expansion of the spirit 396,75; and in cooling down from 100° to 30°, 398,0; the difference is 1,25: but I have already shewn that this difference is not so great as it would have been had it not been cooled down from 60° to 30°: if therefore we say, as 232 : 1,25 :: 164 : 0,88, it is evident that the latter quantity must be subtracted from 396,75, and there will remain for the total expansion of the spirit by this instrument, in heating up from 30° to 100°, 395,87, which is different from the experiments by weight 1,68 division, in defect.

The following are experiments made with the same instrument, and a mixture of equal parts of spirit and water, being some of the same which was used in the former experiments.

Having charged the instrument with this mixture, it was immediately put into a vessel of water, whose temperature was 60°, and the mixture was found to stand in the two tubes at 3,5 above 0. I then cooled it down to 30°, and it sunk to 122

below 0. I heated it up to 100°, when it was found to rise to 188 above 0. It was afterwards brought to the temperature of 60°, and suffered to remain in that temperature for three hours, when it was found to stand at 6 above 0.

It appears from the above experiments, that the contraction in cooling down from 60° to 30°, is 122 + 3,5 = 125,5; and on heating it up to 100°, we have for the total expansion from 30° to 100°, 122 + 188 = 310; but it is obvious that this total expansion cannot be the true one; for it appears, on suffering the instrument to remain three hours in the temperature of 60°, that it was found to have collected a quantity = 2,5, that had undoubtedly adhered to the upper part of the tube when charged, and the fluid having arrived at the temperature of 60° sooner than what adhered could descend, it was of course left behind on cooling the mixture down to 30°; if therefore that quantity had been collected while it remained at the temperature of 60°, it would have stood at 6 above 0, and the contraction from 60° to 30° would have been 119,5 + 6 = 125,5 below 0; and the total expansion from 30° to 100°, = 119,5 + 188 = 307,5 instead of 310, as was found before; and differing from the experiments by weight 3,07 divisions, in defect.

The same mixture having been suffered to remain in the instrument, which was hung up as before, the following experiments were tried two days afterwards.

Having brought the mixture to the temperature of 60°, it was found to stand in the two tubes at 5,5 above 0. It was cooled down to 30°, and was found to sink to 120 below 0. I then heated it up to 60°, and found the mixture in the two tubes to stand at 5,5 above 0 as before. It was afterwards

heated up to 100°, when the mixture in the two tubes was found to have risen to 187 above 0. I again cooled it down to 30°, and found it to stand in the two tubes at 122 below 0. Lastly, I heated it again up to 60°, and found it to stand in the two tubes at no more than 4 above 0.

From the above experiments it appears, that the contraction of the mixture in cooling down from 60° to 30°, is 120 + 5,5 = 125,5; and the total expansion on heating up from 30° to 100°, including the error arising from cooling it down from 60° to 30°, will be 120 + 187 = 307; but in cooling down again from 100° to 30°, we shall have for the contraction 187 + 122 = 309. The former quantity of 307 errs from the experiments by weight 3,57, and the latter 1,57 division, in defect.

But by taking a mean of the quantities found on heating the mixture up from 30° to 100°, including the error arising from some of the fluid being left adhering to the tube, in cooling down from 60° to 30° previous to its being heated up from 30° to 100°, we shall have for the total expansion 307,25, and it was found in cooling down from 100° to 30° to be 309; the difference is 1,75; if then we say, as 182 : 1,75 : : 125,5 : 1,21, the last number being subtracted from 307,25, we shall have for the true expansion in heating up from 30° to 100°, 306,04; differing from the experiments by weight 4,53 divisions, in defect.

From what was advanced by Mr. RAMSDEN respecting the accuracy of the two instruments with which the foregoing experiments have been made, there was great reason to expect that different results would have been found. It appears that no dependance ought to be placed on experiments made

with that kind of instrument which has a tube rising from the side of the ball, to be closed with a stopper. More accuracy may undoubtedly be expected from experiments made with the other kind of instrument which has two tubes, because one of the inconveniences attending the former is removed in it: but we have seen that even experiments made with this instrument do not bear the same marks of accuracy as the experiments by weight; nor can this be much wondered at, if it be considered that the trifling error of ,027 of an inch, in constructing the instrument, will produce an error of one division, which is equal to 0,24 of a grain on the quantity contained in our weighing-bottle; and how difficult and uncertain it is in such an instrument to ascertain the exact temperature, by placing a thermometer only by the side of it. It is not uncommon to see the fluid in the expansion instrument, and the mercury in the thermometer, move contrary ways: and I have more than once observed an alteration in the thermometer, of more than half a degree, when no alteration whatever has been produced in the fluid in the expansion instrument. Indeed, on the least reflection it must be obvious to every one, that the changes of temperature of the fluid in a ball of $1\frac{1}{4}$ inch diameter, cannot be expected to be so quick as in one of 0,22 of an inch. It is also of the utmost consequence in making experiments with this instrument, though it will render it extremely tiresome to the experimenter, that it be in continual motion; for should that precaution not be observed, very considerable errors indeed will take place.

I am, &c.

Apartments of the Royal Society,
Somerset Place.

G. GILPIN.

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